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SUBJECTS:

Magnetism in its Relation to Induced Electromotive Force and Current.
BY PROF. ELIHU THOMSON.

On the Relation between the Initial and the Average Efficiency of Incandescent Lamps.
BY W. H. PEIRCE.

The Efficiency of the Arc Lamp.
BY H. NAKANO.

The Spiral Coil Voltameter.
BY HARRIS J. RYAN.

The Personal Error in Photometry.
BY PROF. EDWARD L. NICHOLS.

On Modern Views with Respect to Electric Currents.
BY PROF. HENRY A. ROWLAND.

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A paper read before the American Institute of Electrical Engineers, New York, May 22d, 1889, and discussion thereon.

MAGNETISM IN ITS RELATION TO INDUCED ELECTROMOTIVE FORCE AND CURRENT.

BY ELIHU THOMSON.

There is, perhaps, no subject which at the present time can have a greater interest to the physicist, the electrician, and the electrical engineer than the one which heads this paper. The advances which have been made in the study from its purely theoretical or scientific side, and the great technical progress in the utilization of the known facts and principles concerning magnetic inductions, can but deepen and strengthen that interest.

On the side of pure theory we find the eager collection of experimental data, to be submitted to the scrutiny of the ablest and brightest minds, to be examined and reasoned upon with the hope of finding some clue to satisfying explanations; and, on the side of practice, we find the search for new facts and relations no less diligent, though often stimulated by practical problems presented for solution. Indeed, the urgency for results is often the greater on the practical side, for theory can wait, practice cannot, at least in the United States.

We must look for continued triumphs in both directions, and the most welcome of all will be the framing of a theory or explanation which will enable us to interpret magnetic and electric phenomena. The recent beautiful experiments of Hertz on magnetic waves have opened a fertile region for investigation.

It would seem that the study of magnetism and electricity will give us the ability to investigate the ether of space, which medium has been theorized upon at great length, with the result of leaving it very much where it was before, a mysterious necessity.

Faraday says, speaking of magnetism:

"Such an action may be a function of the ether, for it is not

at all unlikely that if there be an ether it should have other uses than simply the conveyance of radiations." 3,075 Vol. iii., Exp. Res.

"It may be a vibration of the hypothetical ether, or a state of tension of that ether equivalent to either a dynamic or a static condition, etc." 3,263. Vol. iii., Exp. Res.

Faraday again says, speaking of the magnetic power of a vacuum:—

"What that surrounding magnetic medium deprived of all material substance may be I cannot tell, perhaps the ether." 3,277. Vol. iii., Exp. Res.

Modern views would seem to point that through a study of magnetic phenomena we take a feeble hold upon the universal ether. Magnetism is an action or condition of that medium, and it may be that electrical actions are the expression of molecular disturbances brought about by ether strains or interferences. The close relations which are shown to exist between magnetism and light tend to strengthen such views. Indeed it would not be too much to expect that if the mechanics of the ether are ever worked out we should find the relation between sensible heat and electric currents to be as close as that of light to magnetism; perhaps find ultimately that the forms of matter, the elements and compounds are the more complex manifestations of the universal medium; aggregations in stable equilibrium. It is a difficult conception, I confess, and a most shadowy and imperfect one, yet facts and inferences which favor such views are not wanting.

Our science of electricity seems almost to be in the same condition that chemistry was in before the work of Lavoisier had shed its light on chemical theory. Our store of facts is daily increasing, and apparently disconnected phenomena are being brought into harmonious relation. Perhaps the edifice of complete theory will not be more than begun in our time, perhaps the building process will be a very gradual one, but I cannot refrain from the conviction that the intelligence of man will, if it has time, continue its advance until such a structure exists.

I have been led to make these general allusions to electrical theory in order to emphasize the fact that in the present paper no unraveling of the mystery is to be attempted, but rather the presentation of some few considerations upon a subject of absorbing interest.

The conception of Faraday in regard to the existence of lines

of magnetic force representing directions of magnetic strain or tension in a medium, has not only lost nothing of its usefulness up to the present time, but has continually been of great service in the understanding of magnetic phenomena. We need spend no time in showing, as Faraday and others have done, that these lines are always closed circuits, polarized so that the direction of the lines cannot be reversed without reversal of the actions. Nor need we take time to show that in any medium the lines are mutually repellant laterally if of the same direction of polarization. Opposing this tendency to separation or lateral diffusion of magnetic force, is the strong apparent tendency of the lines to shorten themselves in any medium. These actions are disturbed by the presentation of a better medium, as iron, instead of space or air. Lines of force will move into the better medium, having apparently the constant tendency to diminish the resistance in their paths.

The peculiar and mysterious nature of media such as iron, is to permit an extraordinary crowding of lines on account of slight resistance to their passage through them. We need not, in addition, do more than refer to the other well-known facts of an electric current developing magnetic lines encircling the conductor, as being the general type, which includes all forms of magnetic field, or electro-magnets sustained by currents, and the fact of a development when magnetic lines or circuits and material masses are in relative movement, of electromotive forces transversely to the direction of the lines of magnetism and also transversely to the direction of relative movement, as in the case of electric conductors traversing or cutting through a field, or of a field traversing or being moved across a conductor. We must not forget that even insulators, as well as conductors, cutting lines of force, have the electromotive force developed in them. The action simply develops potential difference, and this generates the current where a circuit exists. While we are in the habit of saying that a conductor moved across a field of lines, or *vice versa*, generates electric current, I think the statement incomplete. The movement only sets up a potential difference and the power expended in effecting the movement generates $C \times E$. The current is energy less the potential, or the energy expended gives the two effects of potential or pressure, and current or rate of movement. Consequently, an insulator or an open-circuited conductor, traversing a field, consumes no energy, potential dif-

ference only being produced. Nevertheless, as will be shown, the magnetic circuits or lines themselves may furnish the energy for their own movement across a conductor, and so develop current as well as potential This occurs in the efforts of lines to shorten their paths, to lessen their density, to pass to better media. Indeed, a close examination will show that wherever power is expended in developing current in a circuit, cutting lines of force, the energy expended is first employed in stretching the lines, which thus receive the energy required to permit them, in shortening, to cut the conductor and set up currents in the electric circuit in accordance with the potential difference developed in that circuit and its resistance.

I think we may also say, though I do not remember to have seen the statement so put, that whenever electric potential is set up inductively, as in self-induction, mutual induction, induction from one circuit to another and induction from magnets or magnetic field, it is set up by the movement of lines of force laterally across the body. mass or conductor in which the potential is developed, and that whenever current is set up in a wire or an existing current prolonged, or an existing current checked by induction, self-induction or induction from magnets, the action is a transfer of energy, represented by strained lines of force shortening or lessening their resistance, or lengthening and increasing the resistance in their paths. The magnetic field is like an elastic spring, it can in one condition represent stored energy—can be strained and will store energy—it can be made to relieve its strain and impart energy.

Let us examine some known phenomena in this light. Take the case of a simple wire, conveying currents say in a line away from observer, Fig. 1. There exists a free field of circular magnetism (so-called), shading off away from the wire and which is represented by concentric circles of increased diameter. The superior intensity or strength of the lines near the wire may also be represented by their thickness. This is often shown also by crowding the lines near the wire, though I am disposed to regard Fig. 1 as more nearly expressing the condition, unless we are to regard the lines as simply indicating a sort of atmosphere of magnetic effect whose density becomes less as we proceed outward from the wire, in which case either form of symbol suffices. The direction of polarization of the lines may be indicated by an arrow head pointing in a direction of right-handed rotation in

the path of the lines. This is the typical figure or expression for all forms of simple magnetic circuit—the form of the lines, their length, position, density, will depend on the shape of the conductor or conductors (when more than one), and the materials surrounding or in proximity to the wire or wires.

If the current traversing the conductor is constant, the magnetic field around it is stable and static, unless other influences come in to modify it. The cutting off of the current is followed by instability of the field whereby it can and must produce dynamic effects. I say *must* because the field represents stored energy and in disappearing *must* give out that energy. To throw light on this part of the subject is one of the objects of the present paper. Cutting off the current supply in the case assumed leaves the developed magnetic lines or strains unsupported, they at once shorten their paths or circuits, collapsing upon the conductor as it were, and continuing this action, cut the section of the conductor, and apparently disappear in magnetic closed circuits

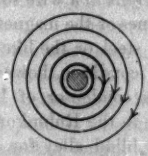

Fig. 1.

of infinitesimal diameter but of great strength of polarization. It appears to me that we must either be prepared to give up the idea of lines of force or take the position that the magnetic circuits precipitate themselves in shortening their circuits, disappear upon and cut the conductor. It was Hughes who put forward the idea that an iron bar in losing its apparent magnetism really short circuits the lines in itself as innumerable strongly magnetized closed circuits among the molecules. In becoming magnetic once more these short circuits are opened or extended into the air by some source of energy applied to strain the lines, such as a current in a conductor around the bar.

May not this idea be extended then, to include the magnetic medium, the ether itself? Does it contain intensely polarized closed circuits of magnetism which are ready to be stretched or extended under certain conditions by the application of energy, which energy is returned by the collapse of the extended circuits?

This is doubtless but a crude expression of the real condition of things, for the lines are only symbols for a condition of strain in a medium which cannot be represented in thought, as we know nothing of its real nature. There is one point in this connection which I must emphasize. The strained lines, Fig. 1, are indications of stored energy in the ether, and the lines *cannot* disappear without giving out that energy. Ordinarily it makes its appearance as the extra-current, and adds itself so as to prolong the current which extended the lines when an attempt is made to cut off such current. Were it conceivable that the current could be cut off and the wire put on open circuit while the lines still remained open or strained, the energy must still escape when the field disappears. It would then produce such a high potential as to be able to discharge from the ends of the conductor, and if the conductor were of some section, part of the energy would be expended in setting up local currents in it. The field could not disappear without an outlet for the energy it represents. But we cannot cut off a current in a wire so as to leave the wire on open circuit while the lines of the magnetic circuit remain around it without having iron, steel or the like in the magnetic circuit. We can approach that condition, however, by breaking the cir cuit very quickly with a condenser of limited capacity around the break. This is done in the Ruhmkorff coil primary; the condenser forms a sort of blind alley for the extra current on its beginning to flow out of the primary coil. But the condenser charges and so backs up and stops the discharge from the primary, even giving a reverse current. The lines of magnetic force collapse, however, and have their effect in the enormous potential set up in the secondary coil.

Take away the secondary coil so as to stop that outlet, the energy expends itself on the iron core and the primary coil. Take away the iron core and the energy of magnetization of the air or ether core expends itself on the wire of the primary and, possibly, also on the dielectric of the condenser to some extent. The extra current becomes in this instance an oscillatory discharge of very high period back and forth through the primary coil from the condenser, until the energy is lost in the heat of $C^2 \times R$. This conversion is doubtless rendered all the more rapid by uneven distribution of current and eddy currents set up in the wire of the coil.

The considerations just given concern the loss of field or the

shortening and apparent disappearance of the magnetic lines or circuits, as giving rise to the self induction or increased potential on breaking. Where the energizing current is slowly cut off or diminished, the energy is gradually transferred to the wire in producing elevation of potential during the decrease; and the collapse and cutting of the wire by the collapsing circuits or lines is then only more gradual.

Let the current be returned to the wire after disappearance of magnetism and the lines again seem to emanate from the wire and at the same time cut it and produce a counter potential in it, which is the index of the abstraction of energy from the circuit, and of its storing up in the form of elastically strained lines of magnetism around the conductor. The effect is that of self-induction on making or upon increase of current, the measure of the amount being the energy stored in the magnetic circuits which have been extended or opened up by the current. The greater the current and the shorter the path for the lines developed around the axis of the conductor the greater the energy stored up. Hence, a circular section conductor has the highest self-induction, a tube of the same section less as its diameter increases, a flat strip has less as its width increases and thickness diminishes, a divided conductor much less than a single conductor of same shape and section. Separating the strands of a divided conductor increases the length of magnetic paths around it and so diminishes the self-induction. A striking instance of this latter fact was developed in conveying very heavy alternate currents of very low potential a distance of about three feet by copper conductors, the current being used in electric welding operations.

The conductors were built up of flat thin strips of copper for flexibility. When the strips were allowed to lie closely together the short conductor showed an enormous self-induction which cut down the effective potential at its ends near the work. By spreading apart the strips so as to lengthen a line around the conductor, the self-induction could be easily made less than 35 per cent. of what it had been before. The interweaving of the outgoing and return conductor strands as one compound conductor, gets rid almost entirely of the self-inductive effects, because neither conductor has any free space in which to develop strong magnetic forces, but is opposed in effect everywhere by the opposite current in its neighbor.

Where a number of conductors are parallel and have the same direction of current, as in a coil or in a strand, it is evident that, statically, the conductor may be considered as replaceable by a single conductor with the same external dimensions and same total current in the area occupied, the magnetic forces or lines surrounding them being of the same intensity. But with changing current strength the distribution of current in the conductor has also a powerful effect on the energy absorbed or given out in accordance with the magnetism produced. Hence the self-induction of a strand, coil or conductor of the same section varies with the rapidity of current changes, owing to the conduction being uneven.

The uneven distribution of current, or its tendency to flow on the outer parts of a conductor when the rate of variation or alternation is made great, is in itself a consequence of the fact that less energy is transformed into magnetism in this case, than when

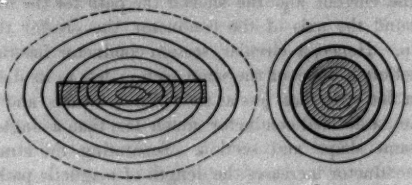

Fig. 2.

the current flows uniformly over the section or is concentrated at the centre. In other words, when a uniform current traverses a conductor of some section the circular magnetism or surrounding magnetic lines are to be found not only outside the conductor, but also beneath its exterior. Since in forming these lines on passage of current the middle of the section would be surrounded by more lines than any other part of the conductor, the current tends to keep out of that part and move near to the exterior in greater amount. Hence in rapidly alternating currents the conductor section is practically lessened, being restricted largely to the outer metal of the conductor. If the round conductor, Fig. 2, were made of iron, the magnetism interior to it and set up by a current in it would be very much greater, the section of the conductor being filled with magnetic circuits or lines around the centre. The total magnetism, external and internal, would be much greater in this case for a given current flow, and

the energy absorbed and given out in formation and loss of field, or the self-induction would be much increased. This could, however, be greatly diminished by slitting the conductor radially or making it of a number of separate wires out of lateral magnetic contact, one with the other, Fig. 3. In these cases the resistance of the interior magnetic circuits would be increased, as there would be several breaks in the continuity around the centre

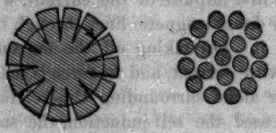

Fig. 3.

of the conductor. The total magnetism which could be set up by a current would be lessened, and the self-induction, therefore, lessened.

The moment we begin the bringing of iron into proximity with an electric conductor conveying current, we provide a better medium for the flow or development of magnetic lines or circuits. In other words, the lines may then be longer, yet equally intense, or more lines may be crowded into a given section of this metal than in space or air. Figs. 4*a*, 4*b*, 4*c*, show the effect brought about by bringing iron of different forms near to the conductor.

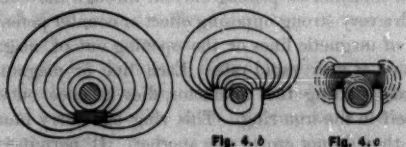

Fig. 4.*a* Fig. 4.*b* Fig. 4.*c*

It shows, in other words, the development of the ordinary electro-magnet of the horseshoe form, and the concentration of the lines in the better medium. The lines also tend to shorten and diminish the resistance to their passage, so that attraction of the iron to the conductor takes place, and if there is more than one piece of iron they tend to string themselves around the conductor in magnetic contact with one another.

When copper bars of one inch diameter are traversed by currents of 40,000 to 60,000 amperes, as in welding them, the mag-

netic forces just referred to become so enormous that very heavy masses of iron brought up to the bar are firmly held, even though the current be of an alternating character, changing direction many times a second.

When a conductor is surrounded by an iron ring, as in Fig. 5, the current in such conductor has an excellent magnetic medium surrounding it. A large amount of energy is then abstracted on the first impulse of the current, which goes to develop strong and dense magnetic lines through the iron ring and across the gap in it. On taking off the current the energy is returned as an extra current, and its force is many times what it would be with air alone surrounding the conductor. We have then greatly increased the self-induction, the storing of energy and opposition to flow at the beginning, the giving back of energy and assistance to the current flow on attempting to remove or stop the current. Let us now complete the ring by making it of iron, endless, Fig. 6, with the conductor in the middle.

Fig. 5.

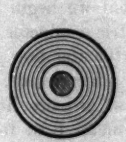

Fig. 6.

We now find that on passing current through the conductor it meets with a very strong opposing effect or counter potential. The evolution of magnetic lines or the opening out of magnetic circuits goes on at very rapid rate. Each line or magnetic circuit evolved and cutting the conductor, flies at once outward and locates itself in the iron ring. This ring can carry innumerable lines, and they do not crowd one another. It permits the lines even to lengthen in reaching it, and yet, on account of its low resistance to their passage, the lengthening is equivalent to their having shortened in other media. We will suppose the current not sufficient to exhaust this peculiar capacity for lines which the iron has. Equilibrium is reached, the conductor has widened out innumerable closed circuits and caused them to exist in the ring still closed, but in iron, not space or ether merely. The current passing has continued its action and storage of energy until, to emit another line in view of the resistance now found in the crowded iron ring, is impossible.

Now, let us cut off the current. We are surprised to find a very weak extra current, a practical absence of self-induction on breaking, or at least a giving out of energy in no wise comparable to the absorption on making. Let us put on the current as it was before. Another curious result. But little self-induction now on making, energy not absorbed.

Now cut off the current again. Same effect as before. Now let us put on the current reversed in direction. At once we find a very strong counter potential or opposing self-induction developed.

The ring has been polarized, or retained its magnetic energy, and we are now taking out one set of lines and putting in reversely polarized lines of force. This done, we break the reversed current without much effect of self-induction. The ring remains polarized and inert until an opposite flow of current be sent through. Iron is then a different medium from the ether.

The ring, once magnetized, must, in losing its magnetism, permit a closure of the lines by shortening. This involves their passage from the iron across the space in the centre of the ring notwithstanding its great resistance to the lines of force. As passage from iron to air is equivalent to lengthening of the lines it is readily seen that such lengthening may oppose more effect than a slight shortening, due to leaving iron for air or space, may give in provoking a closure and disappearance of the lines. Looked at from another standpoint, the lines in the iron may actually require a small amount of initial energy to dislodge them therefrom, so that after being dislodged they may collapse and yield whatever energy they represent.

I must reserve for the future, further consideration of the iron ring, but in thinking upon this matter I am led to imagine that the production of a magnetic line in an iron ring around a conductor may represent a sort of wave of energy, an absorption of energy on the evolution of the line from the conductor and a slight giving out of energy on the line reaching that position of proximity to the iron ring that its passage thereto may be said to be a shortening process or a lessening of its resistance.

The magnetism in air, gases and non-magnetic bodies, being assumed to be that of the ether, this medium shows no such effects as those we get with the ring. It does not become permanently polarized, as does even soft iron under the condition of a closed ring. The iron possesses coercive force, or magnetic rig-

idity, and a steel ring would show more of it. The molecules of the iron or steel take a set. If we were to cut the soft iron ring or separate it in any way, this introduction of resistance of air for ether in the magnetic circuit would cause the lines to collapse and set up a current in the conductor. The energy of the ring would have been restored to the latter. The curious thing is that physically the polarized ring does not present any different appearance or ordinary properties different from those of a plain ring, and will not deflect a compass needle. Its condition is discoverable, however, by the test of self-induction to currents of different direction. As a practical consideration, we may mention in this connection that a self-inductive coil for currents of one direction must be constructed differently from one to be used with alternating currents. The former must have in its magnetic circuit a section of air or the like, or be an imperfectly closed circuit, as it were. The latter should have as perfectly closed a magnetic circuit as can be made. We see here also the futility of constructing a Ruhmkorff coil core on the closed iron magnetic circuit plan, because the currents in the primary are interrupted, not reversed.

The considerations just put forward in relation to the closed iron ring, and its passive character under the condition of becoming polarized, are more important than at first appears. It has been found that the secondary current wave of a closed iron circuit induction coil or transformer, whose primary circuit receives alternating current, is lagged from its theoretical position of 90 degrees behind the primary wave, an additional 90 degrees, so that the phases of the two currents are directly opposed; or the secondary current, working lamps only in its circuit, is one-half a wave length behind the primary, instead of only a quarter wave length, as might have been expected.

But when it is understood that the iron core polarized in one direction by the primary impulse does not begin to lose its magnetism when that impulse simply weakens, but waits until an actual reversal of current has taken place, it will be seen that the secondary current, which can only be produced when magnetic lines are leaving the core and cutting the secondary coil, or when the lines are being evolved and passing into the core from the primary coil, will have a beginning at the moment the primary reverses, will continue during the flow of that impulse, and will end at substantially the same time with the primary impulse,

provided the work of the secondary current is not expended in overcoming self-induction, which would introduce a further lag. Moreover, the direction of the secondary current will be opposite to that of the primary, because the magnetic circuits which are opened up by the primary current in magnetizing the core, or which are closed or collapsed by it in demagnetizing the core, will always cut the secondary coil in the direction proper for this result. Transformers of the straight core type with very soft iron in the cores and with not too high rates of alternation should approximate more nearly the theoretical relation of primary and secondary waves, because the magnetic changes in the core are capable of taking place almost simultaneously with the changes of strength of the primary current. This fact also has other important practical and theoretical bearings.

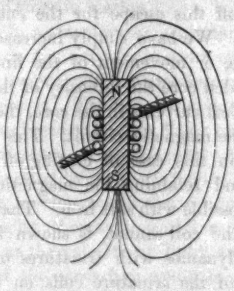

Fig. 7.

Let us assume a plain iron core, Fig. 7, magnetized as indicated, so that its poles N S complete their magnetic circuits by what is called free field or lines in space around it. Let a coil of wire be wound thereon as indicated. Now assume that the magnetism is to be lost or cease, either suddenly or slowly. An electric potential will be set up in the coil, and if it has a circuit, work or energy will be produced or given out in that circuit, and in any other inductively related to it. Hence the magnetic field represents work or potential energy. But to develop potential in the wire the lines must cut the wire. This they can do by collapsing or closing on themselves. The bar seems, therefore, to lose its magnetism by gaining it all, and in doing so all the external lines of force moving inward cut the wire. The

magnetic circuits shorten and short circuit themselves in the bar, perhaps as innumerable molecular magnetic circuits interior to the iron medium. To remagnetize the bar, we may pass an electric current through the coil. The small closed circuits are again distended, the free field appears, and the lines moving outward cut across the wire coil opposite to the former direction, and produce a counter potential in the wire, and consequent absorption of the energy represented in the free field produced. As before studied, the magnetism cannot disappear without giving out the energy it represents, even though the wire coil be on open circuit, and be therefore unable to discharge that energy. The coil open circuited is static, not dynamic. In such assumed case the lines in closing cut the core and heat it. Let us, however, laminate the core or subdivide it as far as possible, and we appear to have cut off this escape for the energy. This is not really so, however. We have simply increased the possible rate of speed of closure, or movement of the lines, and so have increased for the divided core the intensity of the actions of magnetic friction and local currents in the core, the latter still receiving the energy of the magnetic circuit. This reasoning is based on the possibility in this case of cutting off the current in the magnetizing coil and retaining the magnetic field. This is of itself probably impossible with soft iron. That the core receives the energy when the coil cannot, is shown in the well-known fact that in some dynamos with armatures of bobbins on iron cores, the running of the armature coils on open circuit gives rise to dangerous heating of the cores, and that under normal work the heating is less. In the former case the core accumulates the energy represented in the magnetic changes. In the latter the external circuit of the machine and its wire coils take the larger part of the energy which is expended in doing the work in the circuit. In this case, also, the current in the coils causes a retardation of the speed of change and extent of change of magnetism in the iron cores, which keeps down the intensity of the magnetic reaction. In fact, this retardation or lag and reduction of range of magnetic change may in some machines be made so great by closing the circuit of the armature coils themselves or short circuiting them, that the total heat developed in the cores is much less than under normal load.

I wish now, in closing, to refer briefly to phenomena of moving lines of force, and to the effects of speed of movement. In

order to generate a given potential in a length of conductor we have choice of certain conditions. We can vary the strength of field and we can vary the velocity. We can use a strong field and slow movement of conductor, or we can use a weak field and rapid movement of the conductor. But we find also that where the conductor has large section it is liable to heat from eddy currents, caused by one part of its section being in a stronger field than another at the same time. One part cuts the lines where they are dense and the other where they are not dense, with the result of difference of potential and local currents, which waste energy in heat. We cannot make the conductor move in a field of uniform density, because it must pass into and out of the field. The conditions just stated are present in dynamos for heavy current work, where the speed of cutting of lines is low and the armature conductor large in section.

But we find that in a transformer secondary we can use very large section of conductor, even (as in welding machines) 12 to 15 square inches solid copper, without meeting appreciable difficulty from eddy currents in it. The magnetic lines certainly cut the heavy conductor and generate the heavy current and potential needed. What difference, if any, exists? In the transformer the currents are generated by magnetic field of very low density, in which the lines are moving across the conductor with extreme rapidity. The velocity of emanation of lines around the primary coil is probably near the velocity of light, and each line passes across the section of the secondary conductor in a practically inappreciable time. There is no cause then for differences of potential at different parts of the section of the heavy secondary. To avoid eddy currents in large conductors and generate useful currents in them, we may cause the conductor to be either moved into and out of a low density field with very great speed, or better, we may cause the lines of a very low or diffused field to traverse or cut across the conductor with very high velocity.

It is a known fact that in dynamos with large section armature conductors there are less eddy currents produced in the conductors when they are provided with iron cores or wound upon iron cores, than when the conductors are made into flat bobbins moved in front of field poles. Projections existing on the armature between which the conductors are placed have a like effect, and enable us to employ heavy bars or bundles of wire without much difficulty from local currents. The reason is simple. In the

armatures with coils without iron in them or without projections extending between the turns, the conductor moves into and out of a very dense field at comparatively low velocity, so that any differences of potential developed in the parts of the section of conductor have full effect and abundant time to act in setting up harmful local currents. In the cases in which iron projects through the coil or conductor the real action is that the lines of the magnetic circuits move at high speeds across the conductor, and the conductor is at all times in a field of very low density. Figs. 8 and 9 will make this plain. In Fig. 8 we have shown a smooth armature surface having a heavy conductor laid thereon, and which is at *a* just entering a dense field at the edge of the pole N and at *b* leaving the field. It will be seen that when in such a position the conductor, if wide, is subjected to varying field strength and moves at a low speed for the generation of the

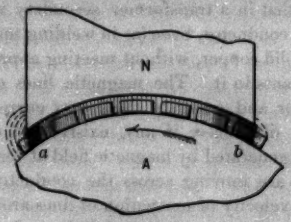

Fig. 9.

working potential as it passes through the field, thus giving rise to eddy currents in the conductor.

In Fig. 9 the conductors are set down between projections, in which case both armature and field poles are laminated or subdivided. As each projection leaves the edge of field pole N the lines which it had concentrated on and through it snap backward at an enormous speed and cross the gap to the next succeeding projection on the armature, cutting the whole section of the heavy armature conductor at practically the same instant. This brisk transfer of lines goes on from each projection to the succeeding one in front of the field pole, leaving a very low density of field at any time between the projections. The best results would be obtained when the armature conductor does not project beyond or quite fill the depth of groove between the projections. Of course there are other remedies for the eddy current difficulty,

notably the stranding and twisting of the conductor on the armature so as to average the position of the parts of the compound conductor.

Perhaps the most extreme case of what may be called dilution of field by projections and by closed magnetic circuits in trans-

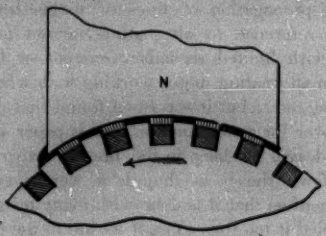

Fig. 9.

formers would be that of a block of iron, B, Fig. 10, moved between poles N and S, and having a hole through it, into and through which a conductor is carried. The path through the iron is so good that we can scarcely consider that any lines cross the hole from N to S; yet as B moves forward there is a continual snapping transfer of lines from the right forward side of the hole to the left or backward side, cutting the conductor as they fly

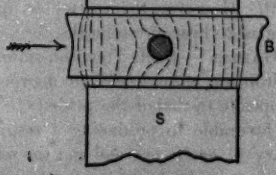

Fig. 10.

across, and developing an electromotive force in it. I have described this action more in detail because we have in it whatever distinction in the manner of cutting the lines of the field is to be found between wire on smooth armatures and on projection armatures and modifications thereof; and also between flat, open

coils passing through a field and bobbins with cores of iron. The considerations advanced also bring out the relation which exists between closed iron circuit transformers and closed iron circuit (projection) dynamos, as we may call them.

I had intended at the outset of this paper to deal to some extent with the propagation of lines of magnetism undergoing retardation in reference to alternating current motor devices, transformers with limited secondary current, or constant average current, an alternating motor working with what I may term a translation lag, etc.; but it was soon found that these matters must remain over for a continuation of this paper at some future time. My endeavor has been in the present paper to deal with the lines of force theory as though it were a symbol of the reality, but I confess that it is done with many misgivings that I may have carried it too for. Yet, if we are to use the idea at all, it has seemed but right to apply it wherever it may throw any light on the subject or assist in our understanding of phenomena.

DISCUSSION.

THE CHAIRMAN (Mr. Edward Weston):—Gentlemen, we have had the pleasure of listening to a very able and interesting paper by Prof. Thomson, and I am a little sorry that we have not adopted the plan of circulating the papers prior to reading them, so that every one might be fully prepared to discuss papers of this character. A paper like this requires considerable thought in its preparation, and to discuss it intelligently necessitates considerable thought. However, the matter is open for discussion and we shall be glad to hear any remarks.

PROF. M. M. GARVER:—Prof. Thomson states that the phase with the closed iron was thrown back 90 degrees farther so that it was exactly opposite to that of the primary. That would furnish conditions favorable to producing a magnetic rotation by using the primary to excite one coil and the secondary to excite another at right angles.

PROF. THOMSON:—No; the angular position in this case would be 180 degrees, not 90. The theoretical position is 90 + 90, which gives 180 degrees lag. That does not give you what you desire.

DR. OTTO A. MOSES:—I listened with a great deal of attention and much pleasure to the very able paper of Prof. Thomson, and

admire it exceedingly. But one of the strong cautions that Faraday gives, is that we should be very careful how we build scaffolding, because we have to take it down afterwards. There is an interesting hypothesis—I might almost call it nebular hypothesis—that emanated from Prof. Thomson which was at first blush exceedingly attractive, where he speaks of the expansion and drawing together again of certain rings around a conductor. That is very pretty at the first glance, but there seems to me one insuperable objection, to which I would like to call his attention. Perhaps he may remove it from me instantly. But I cannot conceive of a line of force closed upon itself expanding and producing flow. When the electric force emanates from a conductor in the form of a ring how can it by a recession upon itself cut lines of force and produce a flow? There would be a flow in one half of the line and there would be a corresponding negative flow in the other half. So that that hypothesis, in my mind, does not furnish a satisfactory explanation of the flow which he tried to describe.

Prof. Thomson :—I do not understand to what flow you refer particularly in this case—the flow of the lines or the flow of the current?

Dr. Moses :—The flow of the current engendering as you say this action, which twined back as it were upon itself, produces the current.

Prof. Thomson :—I do not think I have advanced any explanation of just exactly how that effect is brought about. I was dealing with the effect itself. There is something which we call the magnetic field indicating strains in the ether. Those strains appear to me to be relieved by collapsing upon a conductor. Now, it is a fact that when they collapse they give energy to the conductor, how, I don't know. I do not think I advanced any reasoning on that point.

Mr. Wetzler :—I do not like to prolong this discussion seeing that there are other papers to be read, but I merely rise to remark that some four or five years ago, in a lecture delivered before the New York Electrical Society, Mr. Mailloux, in attempting to explain the manner of electro-magnetic induction, employed an analogy somewhat similar to that which Prof. Thomson has just brought before us, likening lines of force to springs and the snapping of them on the passage of the conductor. Possibly he may have a few words to say on the subject.

Mr. C. O. Mailloux:—I thank Mr. Wetzler for recalling the circumstance. I did deliver a lecture before the Electrical Society, in 1883, which was reported at some length in the *Electrical World* of March 10th. That gives a fair idea of the principles which I advanced at that time. I also sent a contribution some time previous to that, to the London *Electrician*, published in July, I think, 1880, which briefly refers to the fact. The conception came to me as the result of some original investigations, which I made in 1879, while investigating a phenomenon which was thrust upon the world by an itinerant inventor by the name of Garey. He brought out a peculiar magnetic phenomenon wherein, he thought, he had observed a neutral point in the magnetic field not far off from the pole. I wrote a paper on the subject which was illustrated with some carefully prepared magnetic spectra, and I must acknowledge that the paper was sent and returned to me as not being considered fit for publication. Whether the subject was too far advanced for their ideas (laughter) or what was the matter, I do not know. At any rate I received a polite letter stating that they did not care to use the matter, and I still have the paper. I have often thought that perhaps there was a chance to immortalize myself in a mild way in seeing what has come out since, and I have several times started out to publish these spectra. One or two of them will be found published in the *Electrical World* with the report of that lecture. The others I still have in my possession.

I would say that it is too late for me now to enter at any length into the discussion of Prof. Elihu Thomson's paper, but, that perhaps, it will be better for me to send in a contribution as a sort of appendix to his paper. There are some points in which he and I would be probably found to differ. I have believed in the theory of the elasticity of lines of force for some years. I employed the term resiliency in my lecture, and I still adhere to it, as being in my opinion, somewhat more appropriate, and I have believed for a long time that when the mathematical theory of elasticity shall have been developed to a sufficient point that we shall have all of the data necessary for working out a perfect theory of magnetism. In other words, I believe that magnetism is nothing more than a manifestation due to the elasticity of the surrounding medium; that it is an elastic disturbance, and I may state in passing, that it is here that I differ with the conception advanced by Faraday and Prof. Thomson as to the shortening of

the lines of force. I think that the idea of shortening is rather vague. I hardly think that we can say that lines of force shorten as such. It would probably be more appropriate to express it in some other way. What seems to us to be shortening is probably merely the manifestation of some other phenomenon.

Another point is the snapping of the lines of force. I shall endeavor to present some experiments which illustrate the fact that lines of force do snap and break, as I did in the lecture of 1883, and that the snapping is not simultaneous.

I will say in justice to Prof. Thomson, that he has elaborated the conception far beyond what I have done myself; and that he has pointed out conclusions which are perfectly logical, and which do not appear to me to be questionable for one moment, and which ought to be of the greatest theoretical and industrial value, and that I consider the paper which he has presented to us to-day as one of the most valuable papers, without any disparagement to the others.

Mr. J. P. Wintringham:—I would like to ask a single question. In the closed iron ring the magnetism is retained in store and it is not destroyed until a contrary current is sent through the wire; suppose there was a very small current in the contrary direction, would the magnetism of the ring suddenly collapse at some certain point?

Prof. Thomson:—Hardly, I think. There might be a weakening due to the passage of the current, but until the current becomes of sufficient power to practically overturn all the lines, the sudden collapse could not take place. It is true that there is a good field for experiment, to see just exactly how much current would be necessary to take out the magnetism of a closed ring. It would depend, I think, on the coercive force present. If the iron were hard, a larger current would be needed than in the case of exceedingly soft iron, which approaches perfect freedom of motion as far as these lines go.

Dr. Louis Duncan:—I made some experiments on that question. In the first place, with alternating currents we used a transformer and an induction coil of about the same capacity that Prof. Thomson spoke of, one with closed and the other with open circuit. We got very much the result that he predicted in theory. That is, the difference in phase in the closed circuit transformer was about 180 degrees; in the other it was about 90. As to the collapsing of the lines of force, it is gradual. After

the current begins to reverse, the number of lines of force at first gradually decrease, and then decrease faster; but it takes about one-fourth of the force with soft iron that it takes to saturate the iron.

Mr. Geo. H. Stockbridge:—I shall not attempt to discuss Prof. Thomson's paper from a scientific point of view, but it seems to me that it is of interest on other grounds, as being a theoretical treatment of an important subject before the American Institute of Electrical Engineers. Such papers have been presented here before, it is true, but they are out of the line of most of our work here in America and are, for that reason, noteworthy. Allusion was made at the meeting last evening to the fact that electrical science has been developed in this country in the factories and not in the laboratories, and that the commercial interests involved have been at war with the scientific.

This has been undoubtedly a striking characteristic of our work, and has led to the charge that our methods are hap-hazard and unscientific; and in view of the silence of even our best known inventors on purely scientific phases of their work, the charge is not to be wondered at. One who is on the ground can understand why the consideration of patent rights and business emulation should keep the mouths of inventors closed, but to an outsider it is not so clear. These business considerations, again, are of far greater importance in a country where the people, less fettered by tradition, eagerly demand every improvement, and so provide rewards for him who is first in the field.

While these facts explain the nature of our inventive record, it is to be feared that the conditions made necessary by the facts —I mean the conditions of silence and secrecy and the hurry to get ahead of somebody else—have tended to make our methods *really* unscientific and hap-hazard. Some of our successful electrical inventors have learned all they ever knew in the factory, and have gone on to a happy-go-lucky success without much idea of what they were doing and what was the force with which they were dealing. It is certain, at least, that the hurried methods induced by competition have produced with some, the absolutely unscientific habit of neglecting to make a proper record of experiments. There is no excuse for lighting one's cigar with a ten dollar bill, and it is equally unpardonable to neglect to preserve for the benefit of others the details of an experiment which did not happen to prove of value to one's self.

This state of things makes its own comment upon a paper like Prof. Thomson's. If any of you should take the pains to look back through the files of the Institute's transactions, you cannot fail to note that the majority of the papers dealing with theories have been presented to the Institute by those bearing, like the reader of the present paper, a title indicating the teacher. Profs. Anthony and Rowland, Dr. Duncan and Prof. Nichols are names which will occur to you at once. In other words, the results show that laboratory training is, after all, the one which gives us the instinct toward a study of the laws, and a desire to formulate them for the general benefit. It is a matter of congratulation that the instinct and the desire have remained with Prof. Thomson, even after a protracted training in the bad scientific school of the factory.

THE CHAIRMAN (Mr. Weston):—I would like myself to make a few observations. The subject is a very interesting one, and I do not like to cut the discussion too short, but we have not very much time left. As I understand Prof. Thomson, he does not intend to present any new theory to explain the mechanical processes or movements that take place in the production of a current or a magnetic circuit. He takes advantage of the previously existing theories and brings before us a mass of facts of great importance in relation to the practical application of the principles involved. I doubt whether any one has had as clear a conception of the action of the transformer, as has been presented here to-day. Lines of force are not new, nor is their study a new subject. Faraday was not the originator of them, as is commonly supposed; but Sturgeon: one of the men who did a vast amount of useful work in the early history of electrical science, but whose name is very seldom seen or referred to. He was so overshadowed by Faraday's researches, living at the same time as Faraday, that his work has never been fully appreciated. He, I think, was the first man to call them lines of force. I may be mistaken about that. It may be that there was the equivalent of the lines of force in the work of the celebrated Gilbert. His description and also his cuts indicate that he had a pretty clear conception of the subject, but it appears Sturgeon is entitled to more credit than is generally given him. The question as to what is the actual mode of action of these forces is one that we cannot penetrate or appreciate fully yet. Science has got to wait, probably for some centuries. We are getting clearer notions of the

matter, for we have now notions which did not exist a few years ago. We have got a clear conception that there is such a thing as a magnetic circuit and such a thing as magnetic resistance. We have got a notion that there is such a thing as an inductive circuit and inductive resistance, and we have got a notion that there is a conductive circuit and conductive resistance. Now, those terms are employed in a rather free manner, because there is very little doubt that the actions themselves are very closely allied and related to each other. These notions have, however, helped us in our practical work a great deal. That we shall be obliged to abandon finally the lines of force theory, I think is likely. It is simply a temporary expedient to enable us to get a fair conception of certain actions that do take place in accordance with certain laws; just as the atomic theory in chemistry will disappear unquestionably as the mathematical relations of the elements to each other become more clearly understood. They are useful hypotheses, but only hypotheses, and the application of them is not injurious. The subject is too large to discuss very fully. I had made quite a number of notes, but I shall have to stop and tender the chair to Prof. Thomson.

PRESIDENT THOMSON here took the chair and said:

I would like to make an announcement which I believe is in accordance with the ideas of the Institute, that where there is not a chance for discussion and the time is short, it would be just as well to adopt the plan that is often adopted under similar conditions in other societies; to have each one write what he wishes to say on the subject and turn it in to be published in the proceedings. I think that would enable us to get more matter into a discussion, and often times carefully selected matter.

MR. WESTON:—I would suggest at the same time that it would be very much easier for those who take part in the discussion if they had copies of the papers. It is very difficult with the pressure of other matters to keep in mind all the points that have come up, unless you have the papers before you.

THE PRESIDENT:—Many of the societies have adopted that plan and they find it works with excellent satisfaction, and I think we will have to come to it.

A paper on the Relation between the Initial and the Average Efficiency of Incandescent Lamps was then read by Mr. W. H. Peirce as follows:

A paper read before the American Institute of Electrical Engineers, New York, May 22d, 1889, and discussion thereon.

THE RELATION BETWEEN THE INITIAL AND AVERAGE EFFICIENCY OF INCANDESCENT ELECTRIC LAMPS.

BY WM. H. PEIRCE.

At the present day there is very little reliable published information relative to the variation with age of the economy and light giving power of incandescent lamps. However well this law of variation may be known to lamp experts, the author believes himself to be justified in saying that the majority of electrical engineers are entirely unacquainted with it. The most complete data, and the latest, at least in this country, is that given in a report in 1885, to the Franklin Institute by a committee appointed to test lamps and dynamos. At that time the successful commercial lamp was but two or three years old, and since then the development of lighting by incandescence has been so remarkable as to cause this lamp test now to be almost ancient history. When considering the possibilities of incandescent lighting in depots, offices and shops, the fact of the absence of trustworthy records of late date induced the following investigation to be made by the author in the interest of the Chicago, Burlington and Quincy Railroad Company.

There were in all 94 lamps studied, embracing 4 of the most prominent makes of lamps now on the market. Fifty-nine of these were purchased—15 of three kinds and 14 of one type of lamp. Care was exercised to obtain the commercial lamp. After the preliminary measurements had been made upon these 59 lamps, the local agents for them were invited to make examination of the test room and of the methods pursued, and as an outcome of their visits the remaining 35 lamps were tested upon their solicitation. These last lamps were in every case sent direct from their respective factories especially for this test, which fact, I

am rather prone to construe as a vote of confidence by the various lamp companies in our system of measurements.

The Howell voltmeter was used for determining the volts in all measurements, and the following is a brief description of its principle: The pressure to be measured is caused to drop through the resistance A D, Fig. 1, equal to about 4,500 ohms, and a Daniell cell B is so arranged that when the key K is closed the electromotive force between C D is opposed by that of the battery, and hence no current passes through the galvanometer G, when the electromotive force between C and D is equal to that of the cell. In practice the rider C is moved along the resistance

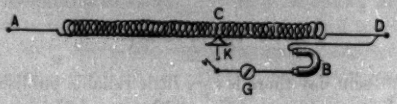

Fig. 1.

until no current flows through the galvanometer, and the electromotive force between A D is then determined from the formula:

$$V = v\frac{R}{r} = 1.08\frac{R}{r}; \text{ where}$$

V—Volts between A D.

v—Volts between C D—electromotive force of cell.

R—Resistance A D in ohms.

r—Resistance C D in ohms.

Naturally the completed instrument avoids this calculation for each observation by having the rider C move along a scale reading to volts. With the instrument used it was found that when the rider pointed to 110 volts that R—4,493 and r—44.4, and hence V — 109.3. Similarly, when the instrument read 100 volts, the true reading should have been 99.4, and so when making observations a correction of .6 volts was applied. The electromotive force of the cells was 1.08 volts, as determined at the laboratory of the Edison Lamp Works. The correctness of our voltage indications being thus dependent upon the battery, a standard liable to variation, it was decided to frequently obtain new cells direct from the lamp company's laboratory. In the early part of the test considerable care was exercised in comparing the voltmeter with four or five others at hand, and just before starting the duration test, four new cells were obtained, and at intervals others

were added, as shown by the following table, until ten in all were had. In table No. 1 the cells are numbered from 1 to 10, in the order of their obtainment. Those cells which checked with one another are marked O. K. The last received cells were always assumed to be correct, and where the old cells differed from them the amount of variation is given in per cent. of the electromotive force of the cell.

TABLE No. 1.

Cell Number.	Dec. 27, 1888.	Jan. 8, 1889.	Jan. 15.	Jan. 26.	Feb. 11.	Feb. 27.	March 14.	April 1.	April 29.
1.....	O K.	O. K.	O. K.	O. K.	O. K.	O. K.	½% low	1½% low	1½% low
2.....	O. K.	O. K.	Low	Low	Low	Low	½% low	½% low	1% low
3.....	O. K.	O. K.	O. K.	O. K.	O. K.	O. K.	½% low	1% low	1% low
4.....	O. K.	O. K.	O. K.	O. K.	O. K.	O. K.	½% low	½% low	1% low
5.....				O. K.	O. K.	O. K.	½% low	½% low	½% low
6.....				O. K.	O. K.	O. K.	O. K.	O. K.	O. K.
7.....							O. K.	O. K.	O. K.
8.....							O. K.	broken	
9.....									O. K.
10.....									O. K.

In this table it is shown that on December 27th and January 8th, all four cells were apparently correct—that is they check with one another. That on January 15th, three cells out of four; January 26th, February 11th, February 27th, five of six cells; March 14th, three of eight cells; April 1st, two of seven cells, and April 29th, four of the nine cells were O. K. And further, that the maximum variations at any time between any two cells was 1½ per cent. It is thus seen that we are reasonably assured that our electromotive force was practically constant.

The cells were mounted on a switch-board, so that any two or all of them could be connected in multiple series by inserting plugs, and they were compared with each other by rapidly substituting first one and then another of the cells in the voltmeter while measuring the electromotive force of the test room circuit. The last two or three cells received were always used when making determinations on lamps.

The ammeter was of the Bergmann solenoid type, having a range of from .2 to 1.0 ampere. The readings of the instrument

were calibrated at the beginning of the test by copper voltameters, the $CuSO_4$ solution of which had a specific gravity approximating 1.16 and the area of the cathode was 4 square inches per ampere measured. The coulomb equivalent was taken as .0003288 gr. The arrangement for calibration was as sketched in Fig. 2, where A is the ammeter, L a lamp to consume the electrical energy, P a voltmeter, and R an adjustable resistance for keeping the pressure uniform at the lamp terminals, as indicated by the voltmeter, and hence to insure constancy of current. The circuit through the voltameters was kept closed for fifteen minutes as timed by a stop watch. Extreme care was exercised in preparing the plates and the preventing of oxidation between weighings. The following table shows the current indicated by the ammeter and that obtained by the voltameters in the last made calibration by copper voltameters.

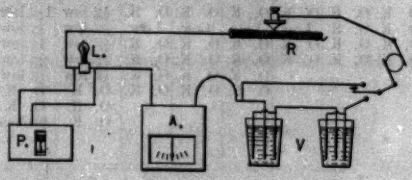

Fig. 2.

TABLE No. 2.

Readings of ammeter.	Readings of voltameters.		Readings of ammeter.	Readings of voltameters.	
.300	.297	.296	.700	.692	.692
.350	.350	.347	.750	.745	.....
.400	.399	.398	.800	.791	.796
.450	.450	.451	.850	.844	.840
.500	.493	.493	.900	.898	.894
.550	.549	.545	.950	.947	.948
.600	.588	.588	1.000	.994	.993
.650	.635	.641			

This table shows that the ammeter was calibrated at every .05 of an ampere from .3 to 1.0 ampere, and that the maximum variation between the voltameter and ammeter readings was about .01 ampere, and that generally the two readings checked to within .005 ampere.

After these determinations the scale was changed to such an extent as was estimated would correct the readings of the ammeter.

The photometer was a 60-inch bar, Letheby-Bunsen, in combination with the Methven two candle slit. The Methven standard was accompanied by a certificate signed by John Methven, stating that a two-candle power light was emitted through the slit, when the quality of the gas used was such as to give from 15 to 20 candles with a flame three inches high. In order to prove the correctness of this standard when using Chicago gas, a comparison with Suggs' standard sperm candles was instituted. Ten different candles were used, two at a time, and the observations were made in accordance with the rules of the London gas referees. Table No. 3 shows that the Methven standard varied less than 1 per cent. from the power of two standard sperm candles; this conclusion being based upon 100 observations.

TABLE NO. 3.

COMPARISON OF METHVEN STANDARD TWO-CANDLE SLIT WITH SUGGS' STANDARD CANDLES.

Series.	Number of Candles.	Brand of Candles.	Grains of Sperm burnt.	Rate of burning Sperm per hour for one candle.	Time of making observations.	Average of ten readings.	Corrected average reading.
1.....	2	Suggs.	40	118.0	10'10½"	1.968	1.936
2.....	2	"	40	114.8	10'28½"	2.034	1.942
3.....	2	"	40	116 1	10'20½"	2.064	1.998
4	2	"	40	114.6	10'27½"	2.010	1.920
5.....	2	"	40	114.8	10'28½"	2.080	1.984
6. . ..	2	"	40	114 9	10'26"	2.140	2.050
7.....	2	"	40	118.8	10' 6½"	2.090	2.008
8... .	2	"	40	119.2	10' 4½"	2.088	2.024
9	2	"	40	114 2	10'31"	2.068	1.966
10.....	2	"	40	113.8	10'33"	2.066	1 960
Grand average of 100 observations.....							1.984

It was recognized that in a test of this kind the four factors most liable to variation were the voltage, the current readings, the standard of light and the personal errors of the observers. It was considered that the most certain and most practical way in which to guard against such errors would be to measure the candle power, the voltage and the current upon several lamps, and to put these lamps aside to be used only as reference lamps.

Accordingly five lamps, which had previously been allowed to burn at an abnormal voltage for nearly 100 hours, had for them the above several quantities determined, and the correctness of the apparatus and personal readings were checked up by the reference lamps whenever observations on the test lamps were about to be made. Table No. 4 exhibits the readings of these lamps. It will be seen that the candle power determinations vary from about 4 per cent. below to 4 per cent. above the first readings made on the lamps, and hence this table proves conclusively that the conditions of the test were essentially the same at all times of its duration. In addition it may be said that for the determination of the candle power of these lamps the mean of five observations at the same point was always taken.

TABLE No. 4.

READINGS OF REFERENCE LAMPS.

Date.	Lamp No. 1 101 volts.		Lamp No. 2 101 volts.		Lamp No. 3 100 volts.		Lamp No. 4 100 volts.		Lamp No. 5 100 volts.	
	C. P.	Amperes	C. P.	Amperes	C. P.	Amperes	C. P.	Amperes	C. P.	Amperes
1.2 89	9.94	.505	9.24	.490	9.40	.480	9.94	.495	9.08	.490
1.8.–	10.10	.505								
1.11.–	10.30	.505								
1.15.–	10 30	.500	9.30	.495	9.40	.483				
1.16.–	10.30	.500								
1 26.–	9.80	.500	9.30	.490						
2.11.–	9 55	.500	8.90	485	9.30	.485				
2.27.–	9.92	.500								
2.28.–	9.66	.500								
3.14.–			9.30	.490						
3.15.–	10.20	.505			9.80	.485				
4.1.–	9.90	.500								
4.29.–	9.70	.505					10.1	.500	9.3	.490

The lamp test was started in August, 1888, at which time all instruments were calibrated, and preliminary measurements were made on quite a number of lamps. On account, however, of press of other work, the test was shortly afterward dropped until late in December. All apparatus and lamps remained during this interval untouched in the test room, and when again taking up the subject in December, the continued correctness of the instruments was proven by the fact that all of some ten or twelve lamps tested out then as previously.

In the preliminary measurements of new lamps the mean horizontal candle power and the watts of electrical energy consumed were determined, and from these were deduced the watts per candle.

In this paper the candle power of a lamp is taken to be the mean of twelve points 30 degrees apart in the horizontal plane of the lamp. The observed candle power is the power of the lamp at a point in the horizontal plane at right angles to the plane of the base of the filament. The reduction factor is the ratio of the mean horizontal to the observed candle power.

Curve of all Lamps, showing
Variation in Economy and Candle Power, with Life.

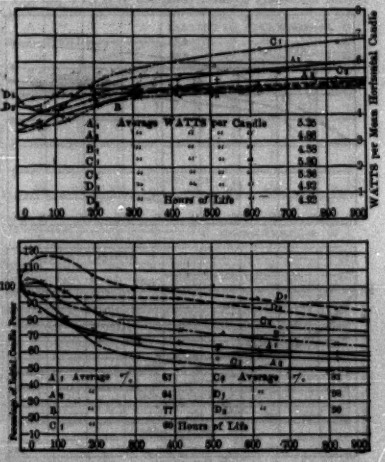

PLATE I

In the preliminary measurements three readings were made at the point where the observed candle power readings were to be made, and the lamp was marked so that this point could always in the future be determined. The lamp was then revolved upon its principal axis, and two observations were made every 30 degrees, the starting point, when again reached, also having two more readings made. The five readings made at the starting point were averaged, and the result called the observed candle-power. The observed candle power and the readings at the other

eleven points were then averaged, and this result called the mean horizontal candle power. In the duration test observed candle power readings only were made, the mean horizontal candle power being always found by multiplying this reading by the reduction factor.

The wire connections to the lamp in the photometer were the same as shown in Fig. 2, excepting that no voltameters were in circuit.

A sliding curtain was hung from the two photometer screen boards, and an opening was maintained only sufficiently wide to

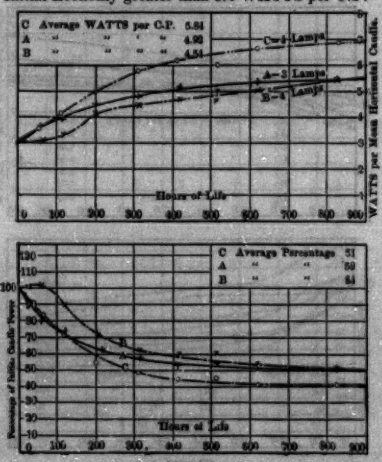
Curves of Lamps with
Initial Economy greater than 3.2 WATTS per C.P.
C Average WATTS per C.P. 5.81
A " " " " 4.92
B " " " " 4.54
C—4 Lamps
A—3 Lamps
B—4 Lamps
Hours of Life
WATTS per Mean Horizontal Candle.
C Average Percentage 51
A " " 59
B " " 64
Hours of Life

PLATE II

permit the reading of the disc. Two screens of deep blue glass were so placed as to shield the person making the candle power determinations from all glare of the lights. Two observers made the measurements. The author made the photometric readings and regulated the height of the gas flame, always looking at the flame through deep blue glass, and an assistant placed the lamps in position, adjusted the voltage, made ampere readings, and recorded all observations.

The candle power readings were always the mean of at least

two observations, and if these two differed from each other more than 4 per cent., several others were made, and a mean of all observations taken as the correct reading.

A small room, 13x19 feet, in which to conduct the test was partitioned off about 40 feet from the dynamos. In this room were located the photometer and other instruments, and all test lamps. The door leading to this apartment was kept locked, no one being allowed to enter during my absence except the engineer in charge of the plant, who twice a day inspected for broken lamps.

Curves of Lamps with
Initial Economy 3.2 and 3.5 WATTS per C.P.

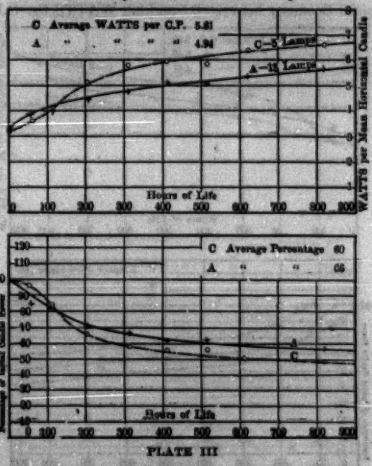
C Average WATTS per C.P. 3.51
A " " " " 4.94
C—5 Lamps
A—15 Lamps
WATTS per Mean Horizontal Candle
Hours of Life
0 100 200 300 400 500 600 700 800 900
C Average Percentage 60
A " " 65
Percentage of Initial Candle Power
Hours of Life
0 100 200 300 400 500 600 700 800 900

PLATE III

The lamps were wired in groups of 10 and 15, those of similar marked volts being placed together.

The volts labeled on the lamps by the manufacturers were taken as being the correct pressure at which to run them. Suitable resistance was introduced into those circuits on which were lamps requiring a less pressure than 110 volts, the normal pressure of the house circuits. No lamps were permitted nearer to each other than 12 inches, for fear of possible harm occuring to one from the heat of the others. The temperature of the air when all lamps were burning was about 104 degrees Fahrenheit, as indicated by a thermometer near the centre of the room. The current was turned off and on the lamps

during the first 550 hours in a gradual manner, such as was due to the starting and stopping of the dynamo, but after this time arrangements were made to also have the lamps switched off and on once or twice daily, so that the filaments would receive the shock of sudden heating and cooling as in usual practice. Excepting when photometric measurements were being made, the lamps burned whenever the dynomos ran, which was generally 9 hours per diem, 4¾ hours in the morning and 4¼ hours in the afternoon, with one hour intervening. In the mains leading to the test room was interposed a variable resistance coil by which the pres-

Curves of Lamps with
Initial Economy between 3.5 and 3.8 WATTS Per C.P.

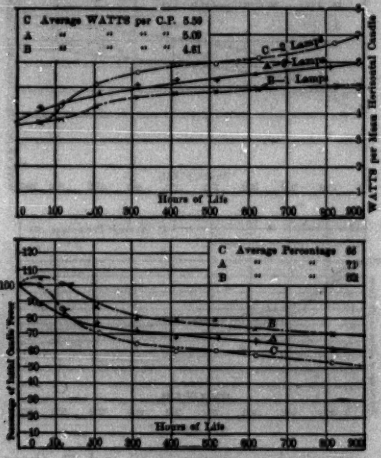
C Average WATTS per C.P. 3.50
A " " " " 3.69
B " " " " 4.81
C—3 Lamps
A—4 Lamps
B—1 Lamps
WATTS per Mean Horizontal Candle
Hours of Life
0 100 200 300 400 500 600 700 800 900
C Average Percentage 86
A " " 79
B " " 62
Hours of Life

PLATE IV

sure at the lamps could be adjusted independently of that of the house circuit. Two pressure indicators were connected with the test lamps, one Howell governing the engineer, and one of the United States E. L. Co.'s pattern in the office of the superintendent of telegraph. In the latter place was a push button with which to ring a bell and thus call the attention of the engineer if the voltage varied immoderately. As a matter of fact, the pressure was generally within one volt of the normal as shown by the indicators, which instruments were from time to time ad-

justed if any variations were found between them and the voltmeter in the test room. A written statement was daily sent in by the engineer as to the exact hours of burning and the failure of lamps.

The duration test was started January 2, 1889, and was continued until April 29, 1889, thus permitting those lamps first entered to burn about 820 hours. The lamps contributed voluntarily by the various companies did not burn this long, because when they were received the test on the purchased lamps was about 200 hours under way.

Curves of Lamps with
Initial Economy between 3.8 and 4.1 WATTS per C.P.

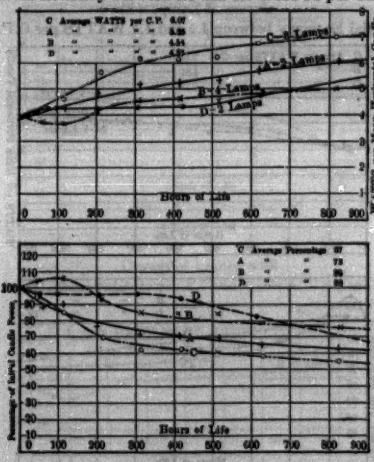
C Average WATTS per C.P. 4.07
A " " " " 4.08
B " " " " 4.04
D " " " " 4.03
C-3 Lamps
A-3 Lamps
B-4 Lamps
D-3 Lamps
Hours of Life
WATTS per Mean Horizontal Candle.
C Average Percentage 97
A " " 73
B " " 89
D " " 88
Hours of Life
Percentage of Initial Candle Power.

PLATE V

It was aimed to make candle-power and efficiency measurements of the lamps after they had lived 55, 110, 200, 300, 400, 500, 600 and 820 hours.

In the accompanying curves each make of lamp is designated by a letter: A_1, B_1, C_1, D_1, have reference to the purchased lamps; A_2, C_2, D_2, indicate those tested at the request of the lamp companies. In either case similar letters refer to like make of lamps.

Three series of curves have been constructed. In the first of these, Plate 1, is shown the variation with age in the economy and candle power of all the lamps. Each curve represents a

batch of from 10 to 15 lamps, thus the c_1 lines relate to the 15 lamps purchased, and the c_2 curves indicate the influence of life upon the lamps contributed by the c lamp company.

The upper set of curves illustrates the economy of the several lamps at their different periods of life. Thus the A_1 curve indicates that these 15 A lamps when new, consumed 3.54 watts per mean horizontal candle, that after 900 hours, 6.1 watts of electrical energy were necessary to produce one candle-power of light, and further that during 900 hours the mean economy of the lamps was 5.25 watts per c. p.

Curves of Lamps with
Initial Economy between 4.1 and 4.4 WATTS per C.P.

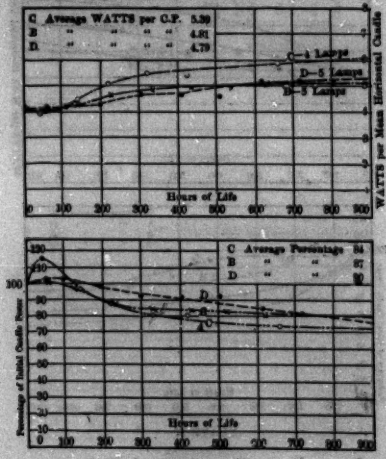
C Average WATTS per C.P. 5.39
B " " " " 4.81
D " " " " 4.79
C—4 Lamps
D—5 Lamps
B—5 Lamps
WATTS per Mean Horizontal Candle
Hours of Life
0 100 200 300 400 500 600 700 800 900
C Average Percentage 84
B " " 87
D " " 92
Percentage of Initial Candle Power
Hours of Life
0 100 200 300 400 500 600 700 800 900

PLATE VI.

The lower diagrams indicate the effect of age upon the light giving power of the lamps. Thus the A_1 curve demonstrates that these A lamps after 900 hours gave but 58½ per cent of the initial candle power, and that the mean intensity of light during 900 hours was 67 per cent. of the new lamps.

In the construction of the second series of curves, Plates II to to VIII inclusive, the lamps have been divided into groups according to their initial economy. We have 7 sets of comparative

Curves of Lamps with
Initial Economy between 4.4 and 4.7 WATTS per C.P.

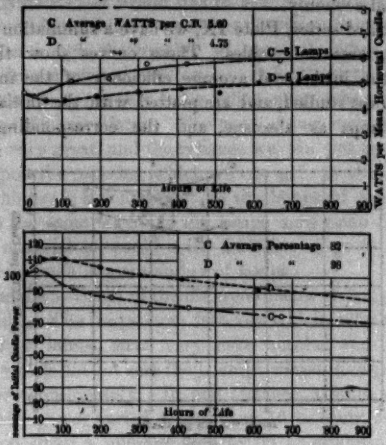
C Average WATTS per C.P. 5.60
D " " " " 4.75
C-8 Lamps
D-8 Lamps
Hours of Life
WATTS per Mean Horizontal Candle
C Average Percentage 92
D " " 98
Hours of Life
Percentage of Initial Candle Power

PLATE VII

Curves of Lamps with
Initial Economy between 4.7 and 5.0 WATTS per C.P.

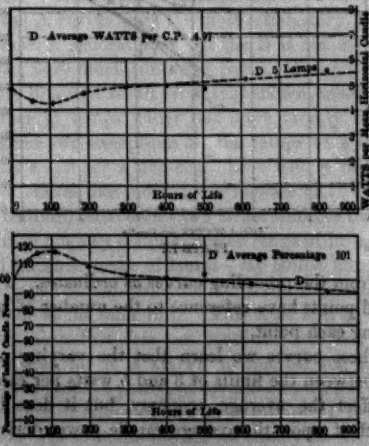

PLATE VIII

curves, the various lamps in each set having approximately the same initial economy.

In the third series, Plate IX, we have a summation of all previous diagrams and tables. These curves show the relation between the initial and average efficiency of the incandescent electric lamps studied, and are plotted with the initial economy of the lamps as abscissæ, and the corresponding averages

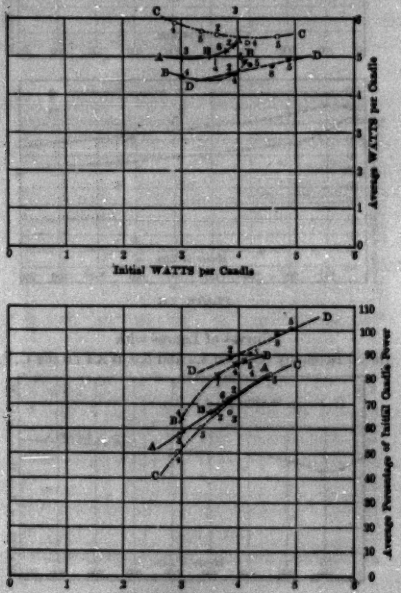
Average WATTS per Candle
Initial WATTS per Candle
Average Percentage of Initial Candle Power
Initial WATTS per Candle

PLATE IX

deduced from the preceding curves as ordinates. The figures at the plotted points have reference to the number of lamps used in determining each point.

From these curves we learn that the varying of the initial economy between the limits of 3 and 5 watts per candle does not greatly affect the average economy, but it does have a very marked effect upon the average candle power derived from the lamp.

DISCUSSION.

PROF. NICHOLS:—I would like to ask Mr. Peirce if he would tell us something about the agreement between individual lamps. It seems as though the number of lamps is rather small from which to draw a general conclusion. It has been the impression that duration tests are only to be relied upon when the number of lamps used is very large. The need of just such work as this I think is very great, and the reliance we can put upon it would depend upon the meaning which we can give to these averages; that is, whether it is found that all the lamps of a single group behave in the same way. If the lamps were made from widely different material it would seem desirable to go to work again with a very much larger number of lamps.

MR. PEIRCE:—Some of the different makes of lamps ran along very evenly. The lamps from factories which make a practice of sorting their lamps, gave very even results, and those from factories which did not sort their lamps would diverge in candle power very considerably. In one instance, a lamp supposed to be 16 candle power varied from about 16 to about 30. I think there is too small a number here to try to establish a law, but there is a sufficient number to show very nearly which way the straws blow. Take for instance the B curve; that is almost a straight line. You have got 20 lamps there. I said that the C, the D and the A curves were the most satisfactory, because they seemed to show a general law; but the B is not so satisfactory, because it is a very much contorted curve. I don't believe that an essentially different result from that shown could be deduced if the number of lamps under test were largely increased, for with but one or two exceptions all the lamps of a single group behaved after a manner which could have readily been predicted from a set of curves like those shown in plate IX. It would, undoubtedly, be more satisfactory if a larger number of lamps were tested, for then not only could a greater range of initial conditions be obtained for each make of lamps—say lamps burning at from 2 to 5.5 watts per candle—but also lamps could be obtained which were not made at the same time. This must not, however, be interpreted to mean that this last condition was not met by the lamps under test, for it is known to me, barring lamps A_2 D_2, that in some instances a period of several months elapsed between the manufacture of lamps of the same group. I believe that if Prof. Nichols will examine the curves closely he will readily discover that uniformity which he is seeking.

A paper read before the American Institute of Electrical Engineers, New York, May 22d, 1889, with an introductory note by Edward L. Nichols, and discussion thereon.

THE EFFICIENCY OF THE ARC LAMP.

BY HATSUNÉ NAKANO.

Note.

The interesting results recently obtained by Mr. Merritt in the application of the method of Melloni to the investigation of the efficiency of the incandescent lamp have made it seem desirable to extend that method to the study of the arc lamp also.

Existing data concerning the efficiency of this source of light are exceedingly imperfect, being based chiefly upon measurements with the Bunsen photometer. Ordinary photometric determinations of the light giving value of sources of illumination differing widely in temperature from the standard candle are necessarily at fault; and I have endeavored in a recent paper[1] to call attention anew to the character of the errors involved in comparison of the light of the electric arc with that of other sources of artificial illumination.

The efficiency of the arc lamp has been expressed hitherto in terms of the candle-power of light produced per unit of energy expended. The estimate has been based in a few instances upon the mean spherical candle-power, as actually determined; more frequently upon the candle-power emanating from the lamp in a single, especially selected, direction, and in too many instances upon the "nominal candle-power." Such estimates of the efficiency of the arc are of small value. They afford us widely varying results which cannot be definitely compared, and even when they are based upon the most complete and careful determinations of candle-power and energy, they do not indicate the ratio of light-giving radiation to the total energy expended

1 The Efficiency of Methods of Artificial Illumination. Transactions of the American Institute of Electrical Engineers, Vol. 6, 1889.

within the lamp. It seemed, therefore to be of some importance to obtain the measurements of the ratio of luminous to total radiation in the case of the arc lamp by a method the results of which would be directly comparable with those already obtained for the incandescent lamp.

The investigation was undertaken, at my suggestion, by Mr. Hatsuné Nakano, M. E., of the Imperial University of Japan, the results of whose measurements are given in the following pages.

E. L. N.

On the Efficiency of the Arc Lamp.

The following experiments were made in the Physical Laboratory of Cornell University during the winter of 1888-89, the object in view being to determine the efficiency of the arc lamp.

It is a well-known fact that the comparison of the candle-power of differently colored lights, such as the candle and the electric arc, is only a rough approximation. By means of one of the methods recently described by Mr. Merritt,[2] however, we can determine the efficiency of the arc independently of its candle-power. The method in question enables us to find the ratio of the light-giving radiation to that of the total radiation of the lamp.

In his first experiments Mr. Merritt measured the heat given out by a lamp, which was immersed in the water contained in a calorimeter. It is evident that this method, though very ingenious, cannot be employed conveniently in the case of the arc lamp. His second method is, however, applicable to arc lamps as well as to incandescent lamps. This method and the arrangement of the apparatus were fully described by Mr. Merritt in his paper, read before the American Association for the Advancement of Science at their meeting in Cleveland. It was also described by Professor Nichols in his paper read before the American Institute of Electrical Engineers, March 12, 1889.

The application of the method to the arc lamp was as follows: The rays of the light were allowed to fall upon the face of a delicate thermopile which was in circuit with a sensitive galvanometer. The deflection of the galvanometer was taken as a

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2 Ernest Merritt: Some Determinations of the Energy of the Light from Incandescent Lamps. *American Journal of Science*, Vol. 37, p. 167.

measure of the energy of the total radiation which fell upon the face of the pile.[3] After the deflection due to the total radiation had been measured, a glass vessel of rectangular shape one decimeter thick, containing a strong solution of alum, was placed between the lamp and the thermopile, and the reduced deflection of the galvanometer observed. The use of the alum cell was to cut off the "dark heat," and allow only the rays of the visible spectrum to reach the face of the thermopile. But a certain small percentage of the longer wave-lengths passed through, and a considerably larger percentage of luminous rays were cut off by the cell. To determine the correction for the former source of error, a cell containing an opaque solution of metallic iodine in bi-sulphide of carbon was placed between the lamp and the alum cell. Any deflection now obtained was due to the dark rays passing through the alum cell, for the iodine cut off the luminous rays entirely, but allowed the dark rays to pass through. It was found that the dark rays which passed through the alum cell were exceedingly weak—almost imperceptible in most cases.

The correction for the second source of error, viz., the absorption of the luminous rays by the alum cell, was determined photometrically. The average of some thirty measurements gave 26 per cent. for the value of the correction.

In my first measurements, the lamp (the centre of the arc) and the thermopile were placed in the same horizontal plane. The lamp used was a "rack-feed" arc lamp requiring 45 volts and 9 ampères for its normal operation. Experiments were made with a dozen or more of different sizes of carbons, ranging from 2-16 up to about 13-16 inch in diameter.

The efficiency, or the ratio of the energy of the luminous radiation to that of the total radiation, luminous and non-luminous, in this plane was found to be exceedingly small, varying from about

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3 John Ericsson in his "Contributions to the Centennial Exhibition," 1876, says that the calorific energy imparted to a thermopile is not proportional to the arcs through which the needle of the galvanometer sweeps, as stated by Melloni, but that for deflections not exceeding 15 degrees, the calorific energy imparted to the pile by radiant heat is very nearly as the square root of the versed sine of the angle of deflection; the deflection of the needle at the termination of the first degree exceeding the energy transmitted, in the ratio of 100 to 89, and beyond 90 minutes the energy becoming greater than the deflection in a constantly increasing ratio. Ericsson does not state, however, what kind of galvanometer he used in his investigations. The one used by me was a low resistance reflecting galvanometer of Sir William Thomson's pattern. It had been tested by Mr. Merritt, who found that the deflections were proportional to the energy imparted to the pile. In my experiments this proportionality was assumed to be true.

5 per cent. to a little less than 15 per cent., the ratio increasing as the diameter of the carbons used decreased. This relation between the efficiency and the diameter of the carbons, which is shown graphically by means of the curve in Fig. 1, ceased to exist, however, when the point was reached at which the whole length of the carbons was rendered hot by the current passing through them. After this point was reached the efficiency fell off again.

Table 1 gives the results of these measurements. In the first column the diameters of the carbons (in thousandths of an inch) are given. The second column shows the "mean horizontal

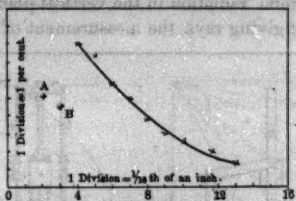


Fig. 1.

efficiency;" that is to say, the ratio of the deflection produced by the total radiation of the lamp to that due to the light-giving radiation alone, after the proper corrections for absorption and diathermancy have been applied. Each value is the mean of a series of observations. The third column gives the potential difference between the terminals of the lamp.

TABLE I.

Diameter of Carbons. (In inches.)	Horizontal Efficiency.	P. D. (Volts)
.882	.01484	42
.780	.02162	35
.605	.02597	43
.563	.03125	38
.500	.03893	38
.432	.05014	38
.370	.05861	28
.305	.07338	31
.250	.0813	56
.185	.0458	25
.124	.0514	30

The curve C D, given in Fig. 1, shows the relation between the efficiency, measured in the horizontal plane, and the diameters of the carbons. The ordinates represent efficiencies, and abscissæ the diameters. It will be noticed that the points A and B corre-

sponding to 2-16 and 3-16 inch carbons, are far below the curve, showing that the relation is modified as soon as the carbons become too small to carry the current without heating.

In the electric arc, as Professor Nichols has shown in the paper already referred to, "the entire light giving area is included between the line surrounding the positive carbon, which is at red heat, and the corresponding line upon the negative carbon. Now the surface of total radiation is much larger than that from which the light-giving rays emanate. It includes in addition to the incandescent surfaces near the arc, all those portions of the carbons which are heated, either by conduction or by the current. Were the distribution of total radiation in the vertical plane identical with that of the light-giving rays, the measurement of their ratio with

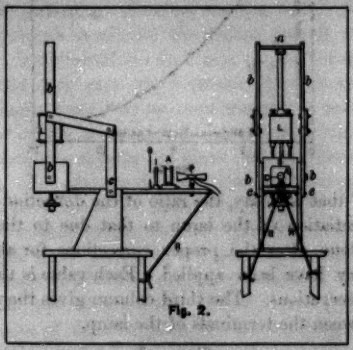


Fig. 2.

the axis of the thermopile in any plane which passes through the arc would give us the efficiency of the lamp, but the curve of distribution of total intensity is not the same as that of candle-power, and the ratio in question is a function of the angle which the axis of the pile makes with the horizon. It becomes necessary, therefore, to make an exploration of the entire zone through which the lamp sends out rays, determining the ratio of luminous to total radiation for each angle and then to integrate the results."[4]

With such an object in view the piece of apparatus which is shown in Fig. 2 was devised. The lamp was hung from a horizontal beam *a*, supported by two upright pieces *b b*, which could be kept always vertical by means of a "parallel motion"

4 Edward L. Nichols. l. c.

arrangement. The alum bath A and the thermopile P were also carried upon a board, pivoted at *c c*, so that it could be turned to any desired angle. All necessary changes of angle

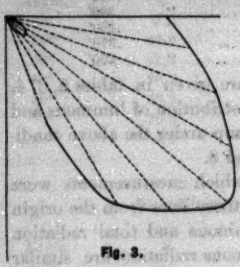


Fig. 3.

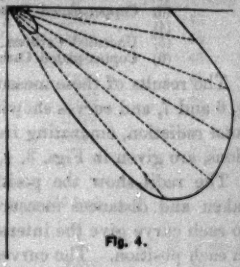


Fig. 4.

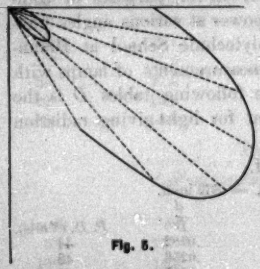


Fig. 5.

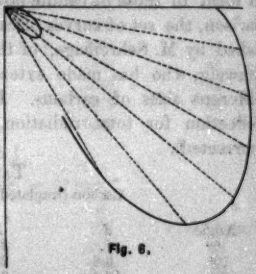


Fig. 6.

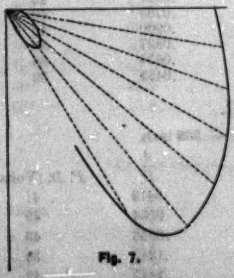


Fig. 7.

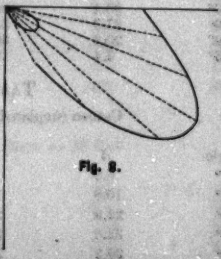


Fig. 8.

could be made without altering the distance between the arc and the thermopile, or the relation of the alum cell to either of them. By means of this apparatus measurements were made at angles of

0°, 10°, 20°, 30°, 40°, 50°, 60° and 63° below the horizonta planes. The following carbons were used:

(1) Uncoated Carbons.....diameter — .882″
(2) " " ..... " .563″
(3) Copper-plated Carbons..... " .500″
(4) " " ..... " .450″
(5) Uncoated Carbons..... " .370″
(6) Copper-plated Carbons..... " .250″

The results of these measurements are given in tables 2, 3, 4, 5, 6 and 7, and curves showing the distribution of luminous and total radiation, emanating from the lamp under the above conditions, are given in Figs. 3, 4, 5, 6, 7 and 8.

The radii show the positions for which measurements were taken and distances measured along these lines from the origin to each curve give the intensity of luminous and total radiation in each position. The curves of luminous radiation are similar in form to those of candle-power. Note, for purposes of comparison, the set of curves of candle-power at various angles published by M. Schreihage,[5] of the Polytechnic School at Braunschweig, who has made extended measurements of lamps with different sizes of carbons. In the following tables D is the deflection for total radiation, d that for light-giving radiation (corrected).

TABLE II.

Carbon (unplated), diam. — .882 inch.

Angle	d	D	$\frac{d}{D}$	*P. D.* (Volts).
0°	4.	220	.0182	44
10°	6.8	265	.0256	42
20°	16.4	300	.0547	42
30°	27.3	350	.0780	40
40°	35.1	390	.0900	40
50°	27.3	330	.0827	38
60°	20.3	300	.0677	39
63°	5.4	125	.0432	39

TABLE III.

Carbon (unplated) diam. — .563 inch.

Angle.	d	D	$\frac{d}{D}$	*P. D.* (Volts.)
0°	6.8	165	.0412	41
10°	10.8	195	.0554	42
20°	22.9	245	.0956	43
30°	33.8	264	.1242	39
40°	39.2	274	.1441	42
50°	28.4	252	.1137	41
60°	10.8	128	.0625	40

5 Schreihage, *Centralblatt fur Electrotechnik*, No. 22, 1888. See also *La Lumiere Electrique*, T. 29, p. 585.

Table IV.

Carbon (copper-plated), diam. — 5 inch.

Angle.	d	D	$\frac{d}{D}$	*P. D.* (Volts.)
0°	5.4	110	.0491	45
10°	12.2	140	.0872	45
20°	21.5	172	.1250	45
30°	29.7	190	.1538	45
40°	33.8	195	.1733	45
50°	20.3	155	.1310	45
60°	4.1	93	.0441	45

Table V.

Carbon (copper-plated), diam. — .45 inch.

Angle.	d	D	$\frac{d}{D}$	*P. D.* (Volts.)
0°	10.8	197	.0548	45
10°	20.3	225	.0901	45
20°	29.7	242	.1228	45
30°	39.2	260	.1506	45
40°	41.9	270	.1552	45
50°	27.3	255	.1059	45
60°	12.2	180	.0676	45
63°	5.4	110	0492	45

Table VI.

Carbon (unplated), diam. — .37 inch.

Angle.	d	D	$\frac{d}{D}$	*P. D.* (Volts.)
0°	10.	180	.0555	40
10°	14.9	190	.0789	42
20°	24.4	200	.1220	38
30°	31.8	217	.1474	37
40°	39.2	230	.1696	40
50°	42.	240	.1750	42
60°	33.8	205	.1659	42
68°	10.8	115	.0869	38

Table VII.

Carbon (copper-plated), diam. — .25 inch.

Angle.	d	D	$\frac{d}{D}$	*P. D.* (Volts.)
0°	13.5	160	.0844	37
10°	22.9	185	.1238	37
20°	39.2	225	.1742	39
30°	45.9	255	.1800	38
40°	41.9	230	.1822	38
50°	29.7	150	.1980	39
60°	4.1	75	.0547	38

The distribution of total radiation differs widely from that of luminous radiation, so that the efficiency expressed by their ratio depends upon the angle of measurement. The values of the efficiency in each case for the various angles at which measurements were made have been calculated. They are shown in the diagram of efficiencies in Fig. 9.

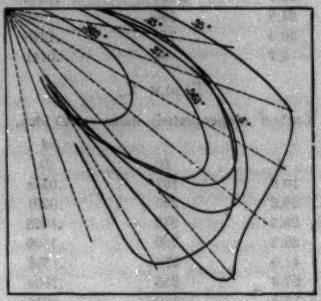


Fig. 9.

In order to determine the mean or "hemispherical" efficiency of the lamp the ratio of the radii of two semi-circles, the areas of which equal the areas of the curves of total and luminous radiation was found by means of a planimeter. The following are the values thus obtained:

TABLE VIII.

Diameter of carbons.	Spherical efficiency.
.833	.0687
.563	.1100
.500	.1266
.450	.1330
.370	.1554
.250	.1660

The above table forms the basis of the curve shown in Fig. 10, which represents the relation between the diameter of the carbons and the "hemispherical" efficiency of the lamp. The diagram is a straight line. Thus we see that the hemispherical efficiency varies inversely as the diameter of the carbon.

It will be interesting to compare this with the result of M. Schreihage's experiments. He measured the mean spherical

candle-power with the different sizes of carbons, and found the same relation to hold true, as will be seen from the following table:

TABLE IX.

M. SCHREIHAGE'S MEASUREMENTS OF CANDLE-POWER AND CURRENT DENSITY.

Cross-section of positive carbon. mm.	Diameter d. m.m.	Spherical c. p.	d. X c. p.	Current density.
40	7.12	417	3,353	0.134
95	11.0	383	3,128	0.066
134	13.6	254	3,462	0.047
194	16.1	189	3,045	0.032
254	18.0	161	2,905	0 25

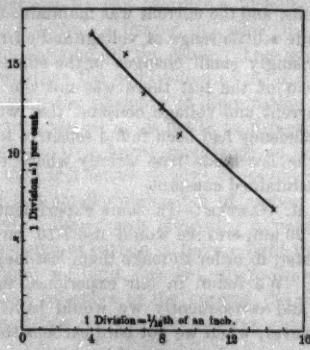


Fig. 10.

M. Schreihage states that this relation is not completely satisfied in the case of the mean horizontal candle-power, especially when the carbons are very small or very large. We have seen that this statement applies also to the experiments upon the horizontal efficiency described in the first part of this paper.

Physical Laboratory of Cornell University, May 6, 1889.

DISCUSSION.

THE PRESIDENT:—The subject of the paper just read is certainly one of considerable importance, and some of the conclusions are unexpected, you may say. As there are persons here having experience in this line, we would like to hear from them.

MR. PRESCOTT:—I would simply like to ask Prof. Nichols if this law that he discovered is independent of the current density. I understand the law to be that the efficiency of the arc lamp depends on the diameter of the carbon without reference to the current density.

PROF. NICHOLS:—I should have said that these experiments were made with constant current and as nearly as possible with constant electromotive force to the arc. The arc was maintained at about 45 volts, and the current was maintained at about 9 amperes; but quite a little range of voltage and current in the lamp showed surprisingly small changes in the efficiency. Probably toward the end of the test there was not the same care taken to hold the current and voltage constant that would have been taken if the efficiency had been found sensitive to changes in this respect, but the law holds true strictly where the current and voltage are maintained constant.

PROF. M. M. GARVER:—In some experiments I made with arc lamps for 20 amperes we would use 7-16 carbon in summer, but in the winter, in order to make them last longer, we used the larger carbon. We found in our experience that the light was not as good, and consequently we would have to increase the current. However, when we got hard carbons they lasted better. We observed, also, in that case, without measurements, that the light was not so good with the hard carbon as with the soft carbon. I would like to ask if anything of the kind was observed.

PROF. NICHOLS:—I would say simply that it was not the intention to go into the question of different carbons. In starting the investigations we simply took such carbons as happened to be available in the laboratory, of different sizes. It was taken up originally as practice work, and I did not know at that time that it would ever come to the light, because I did not expect as good a result as was obtained; but all the carbons were probably carbons of good quality. They were of different makes, American and European.

PRESIDENT THOMSON:—I would add a word just on this point from my own experience. I think, undoubtedly, the density and

hardness of the carbon has an effect on the luminosity, and practical experience, without even referring to refined measurements, is that if a carbon is entirely too hard for the current, the luminosity is so much lessened that people complain at once, although the voltage and current remain the same. I can confirm what Prof. Nichols says also in regard to changes in the length of arc not changing the luminosity very much beyond a certain separation, when the light seems to go out freely, and as it mostly emanates from the carbon ends themselves, a little separation, more or less, will not make a great difference in the effects. It would be interesting in this connection to know exactly what was the combustion of carbon in the different cases. Of course, a part of the cost—the practical cost—is the rate of consumption of carbon, and sometimes we can sacrifice a little light by using a larger carbon and getting a longer life, since the carbon costs something in the running of the lamp. This gives rise to the question as to whether the combustion of the carbon itself does not have to do with the efficiency of illuminating power—whether, in other words, the carbon is not to be regarded in part as a piece of coal, which, if it burns rapidly, gives out more light in a given time than if it burns slowly; consequently with a low current density and small combustion of carbon we lose that effect and depend on the current altogether to produce the light. At the rate of burning of carbon in the arc light there must be a considerable amount of light, due entirely to the combustion of the carbon, without reference to the current, the current merely maintaining the carbon at that temperature which permits this effect to exist.

Mr. Weston:—I understand Prof. Nichols did not make any experiment with any other lamp except this arc lamp. Experience has shown, I think, that the efficiency of an arc lamp is very much increased where the power of the light is increased. If I remember rightly, some experiments I made myself in 1875 and 1876, showed that to a very marked extent, and I think experiments probably earlier than mine showed it. So that the efficiency, as it stands for an ordinary commercial lamp to-day, is not anything near what we can get if we make the lights larger. How far that would extend it is somewhat difficult to say, and as Prof. Nichols has all the appliances for making these measurements, including dynamos, I think it would be a very nice thing if we could get some facts in regard to efficiency of lamps, running up to 100 or 200 amperes instead of 9 or 10. It would

probably throw a great deal of light on the matter. The small amount of useful luminous energy compared to the total energy employed, is a sad thing, always. That, together with the low efficiency of the steam engine shows how much we have to do. There is no lack of work in this field.

PROF. NICHOLS:—I would like to say that in presenting this paper I have presented it not as an exhaustive investigation of this subject, but rather as pointing out a line of work which, I think, may be a good deal of benefit to the study of the light-giving power of various illuminants, and that undoubtedly in order to make it complete it ought to be worked out for the efficiency as a function of the length of arc, independently again, as a function of current, and then those could be combined, so as to give it as a function of the energy for a constant length of arc. In short, it ought to be worked out under all the conditions under which lamps can be used. Here we have only worked it out for a single condition of affairs, because that was convenient to us—the ordinary long arc lamp of to-day.

PROF. GEYER:—May I ask Prof. Nichols whether any attempt was made to see if in the part which he calls luminous energy any appreciable amount, was energy other than luminous—that is, what is ordinarily called actinic. I should think that in a light centre, as an arc light, an appreciable amount might be other than luminous, which would reduce all those figures slightly; but the effect would probably be greater on those which are lowest on the column, and probably less on those which are on top of the column.

PROF. NICHOLS:—Mr. President, I would say that very careful corrections were made for the dark rays—the waves of longer wave length than the red which were able to pass through the alum, but the energy of the ultra-violet rays is so very small when measured on the thermopile that we regarded it as unworthy of mention.

DR. MOSES:—While suggestions are flying thickly, I would like to recall some experiments which might have bearing on future experiments in this direction. On one occasion, in order to get an absolutely uniform light I tried the effect of various colored media on an arc, and I found that with a monochromatic light, the general effect is more uniform throughout different areas; you did not see the sudden sharp flash of light that occurred by the reflection from the crater. Now, if experiments were car-

ried on perhaps by allowing the arc to burn its way through a disc of mica, you would get a perfectly uniform monochromatic atmosphere and that would perhaps be the best medium for making these experiments. Monochromatic light may give uniformity of result.

The following paper on "The Spiral Coil Voltameter," by H. J. Ryan, was then read by Professor Nichols:

A paper read before the American Institute of Electrical Engineers, New York, May 22, 1889, and discussion thereon.

THE SPIRAL COIL VOLTAMETER.

BY HARRIS J. RYAN.

During the present year at the Physical Laboratory of Cornell University, the copper voltameter has been used somewhat extensively in laboratory practice by students for calibrating Thomson graded galvanometers, tangent galvanometers, and other electrical measuring instruments where a single determination suffices as a calibration. For practical instruction students are made to check their results on a Thomson gravity balance, and by means of the large galvanometer at the Magnetic Observatory.

The plate form of copper voltameter has been thoroughly and admirably investigated by Mr. Thomas Gray,[1] of the Physical Laboratory of the University of Glasgow. We are indebted to Mr. A. W. Meikle[2] and to Lieutenant Anderson[3] for clear descriptions, based upon the results of Mr. Gray, for the use of this form of voltameter, whereby determinations may be made so accordant, that one can scarcely think of more to be desired.

With all this, however, it has been our experience that students are slow to obtain accordant results when making their first attempts to follow Mr. Gray's methods. It seemed desirable to make use of a form of voltameter that required the least consistent time and care to construct and prepare for operation. Again, there should be a wide allowable range of current density, through which deposits would be as firm and adherent as possible. It was with a view of realizing this that we finally adopted and made use of the spiral coil form herein described.

A wire coil as a cathode seemed to present many advantages. Wire of high conductivity, good for this purpose, is generally

1 *Phil. Mag.*, 1886 and 1888.
2 *Lond. Elec.* and *Elec. Rev.*
3 *N. Y. Electrical Engineer*, October, 1888.

available in any desired size. A wire can be cleaned properly with great ease and readiness by fastening one end in a vise, holding the free part in one hand and sandpapering it with the other. By beginning at the vise to clean, it is not necessary to touch the cleaned wire with the hands throughout the complete operation of polishing and coiling into a spiral on a cylinder of the proper diameter, about which has been rolled clean paper. In this manner gain and loss coils are constructed.

Figs. 1 and 2 show the final form of voltameter adopted. The coils are hung vertically. The cathode is made of a smaller diameter and is arranged inside and concentric with the anode. The diameter of the anode is made about 3 c.m. to 4 c.m. larger than that of the cathode. The loss coils prepared as above are ready for use, since for well-known reasons their changes of weight are never accordant. The surface of the gain coil must not be touched

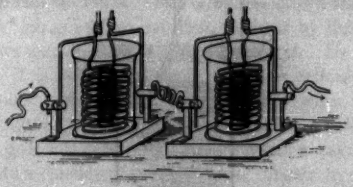


Fig. 1.

by the hand. It is easy to handle it by the extending terminal, by means of which it is suspended in the voltameter. After polishing, it is washed by plunging into a jar of water containing a little H_2SO_4.

It is then rolled on filter or blotting paper to remove all but a mere film of the water. The coil is then dipped in 95 per cent. alcohol, removed and the excess of alcohol allowed to drip into the jar of the same. By rolling the coil on clean filter or blotting paper again, nothing but a mere film of alcohol remains, and that is thoroughly evaporated in a few moments, leaving the coil entirely dry.

Coils that have been laid away and become corroded can be readily cleaned, as is well known and directed, by plunging them into a mixture of strong HNO_3 or HCl, one hundred parts of the former to one of the latter, removing them quickly to a distance, such that the vapors of the acid shall not reach the coil during

the rest of the preparation. This consists in washing, first thoroughly in water, and then proceeding as at first described by plunging in the acidulated water, alcohol, etc. After weighing, the coils are ready for the voltameter.

It has been customary with us to use a coil made of two and one-half metres of No. 16 wire, having the surface area of one hundred square centimetres.

For great strengths of current a number of these are arranged in parallel.

For every four amperes one wire is necessary. At the end of the deposit the gain coils are immediately removed, and plunged first into clean water and then into the acidulated, from which

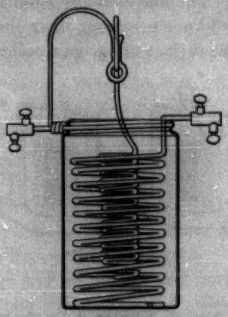


Fig. 2.

they are dried by means of alcohol in the manner above described. When dried they are at once ready to weigh.

The copper sulphate, water and acid, need not necessarily be chemically pure. The density of the voltameter solution should be not less than 1.10 and not more than 1.18.

The question now comes to us as to what degree of accuracy and precision of results we may depend upon from the above form of voltameter.

The successful use of the copper voltameter has come from an appreciation and an understanding of the fact that when copper is immersed in an acidulated solution it passes slowly into solution in an irregular way. Mr. Gray has done much work in this particular, and has shown that the going into solution of copper on the cathode is curiously assisted by the current. From the ex-

tensive work done with the plate form of voltameter at Glasgow results have been gained with regard to the amount of copper that goes into solution for a certain current density and a certain temperature, so that it is a comparatively easy thing to make determinations with assured absolute accuracy to within a tenth or even a twentieth of one per cent. This is remarkable, though nevertheless true.

A Thomson deca-ampere balance was calibrated by means of the copper voltameter under Mr. Gray's direction at Glasgow.

When the same was received and set up at Ithaca in the physical laboratory of the University, it was compared with the large standard tangent galvanometer at the Magnetic Observatory, and found to agree with the same to within one-tenth per cent.

Surely this same or even a greater degree of absolute accuracy could be obtained with less trouble and experience by making use of the spiral coil cathodes. Mr. Gray has shown that the amount of copper which goes into solution, unaided by the current, is so small that we can in almost all cases neglect it. He has shown us how much it is when a plate is used as a cathode. There are strong reasons, however, for believing that when a wire which presents a regularly curved surface, is used as a cathode, the copper from it goes into solution in a more regular way, and probably in all cases to a considerably less degree. Again by the use of the spiral coil in a voltameter we find an advantage in the fact that the plane of each turn of wire is disposed horizontally, so that by convection the solution is not allowed to become weak near the cathode, nor dense near the anode.

This fact was first pointed out by Mr. Shaw, who speaks of the very accordant manner in which platinum wires disposed horizontally are found to act as cathodes in copper electrolysis.

In a valuable paper by Mr. Shaw[4] on "The Verification of Faraday's Law of Electrolysis with Reference to Silver and Copper," the very great range of current densities for the deposition of copper on platinum wires of 8 centimetres to 800 centimetres per ampere has been brought to bear on the ratio of silver and copper deposits when the currents are the same. From data taken from this paper, the writer has plotted curves that show the relation of current density to copper deposit throughout Mr. Shaw's work at the Cavendish Laboratory and Emmanuel College.

4 British Asso. Meeting, 1886. *N. Y. Electrician and Electrical Engineer*, 1887.

The work extended over a year, and was done with much care. These curves are shown in Fig. 3. The ordinates represent the amount of silver deposited by the same current that deposits unit weight of copper in a given time, corresponding to current densities, as represented in square centimetres per ampere on the the abscissæ axis. As is seen, with the exception of two, the curves are in remarkable accordance, differing by not more than 1-34 per cent. With regard to the two curves that fall above and below the rest, we would say that their position can easily be

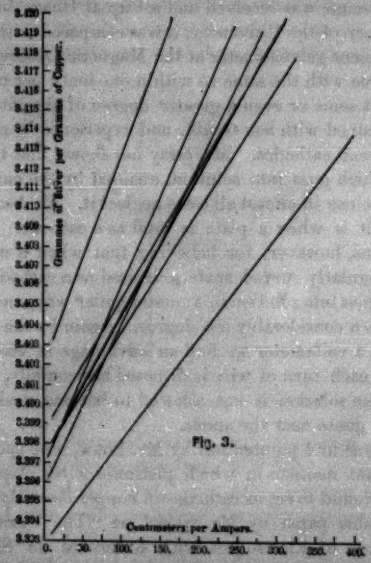


Fig. 3.

attributed to accidental causes; the error in the indications of the silver voltameter of a 1-5 of 1 per cent. either way would account for their position. Again, Mr. Shaw used no acid in the voltameter solutions, a fact that will account for irregularities. Of these lines the heavy one was drawn as a fair average of them all. A glance at this line will derive some interesting results:

We see that with a current density of 50 square centimetres per ampere the ratio of silver to copper deposited is 3.401. In the early part of Mr. Shaw's work, in a long series of observa-

tions, this same identical ratio as a mean for a mean current density the same as above was found.

Mr. Gray when at work on the same ratio determined the same identical value.

Finally the writer has found that if we take the values for the chemical equivalents for silver and copper as quoted by Wurtz and those given by Meyer, that the calculated values for this ratio are 3.401 and 3 399 to 3.401.

Therefore, since we know with great accuracy the value of the electro-chemical equivalent of silver, we have an equal assurance that with a *current density of one ampere* for *every* 50 *square centimetres exposed*, the *amount* of *copper deposited* will be .0003287 *grammes* per *coulomb*.

Again, we see that in a range of current density from 50 centimetres to 300 centimetres per ampere, this line indicates a change in the amount of copper deposited from .0003287 to .0003270. It is to be remembered that these results were obtained by the use of a solution almost saturated, and without acid. Returning to Mr. Gray's work we find that for the same range of current density the amount of copper deposit changes from .0003287 to .0003272 for a temperature at which Mr. Shaw probably worked.

It is well-known that copper goes into solution under the action of the current from the cathode at a much greater rate without acid than with, and especially at a greater rate when the solution is dense than when working under the proper conditions for measuring current. A comparison of Mr. Gray's and Mr. Shaw's results brought us to the belief that less copper should be dissolved from cathodes of the spiral form than those of the plate form.

To determine this, four cells were arranged by Mr. Genung, a student in the Sciences at Cornell. Two of these had cathodes and anodes of the plate form and the other two, those of the "spiral coil" form. The large plate and coil and the small plate and coil had areas of 100 centimetres and 10 centimetres, respectively. About half an ampere was passed through them for a time, amounting to two and one-half hours.

Every half hour they were taken out, dried and weighed, the solutions all intermixed, and the deposits continued for another half-hour, etc.

The solution had a density of 1.1, and an addition of $\frac{1}{4}$ per

cent. acid. An examination of the intermediate results gives the same result that one gets by looking at the total gain of the coils and plates taken separately. Mr. Genung's results were as follows:

Temperature, 20 degrees.

Coils.		Plates	
Small	Large.	Small.	Large
10 c.m.	100 c m.	10 c.m.	100 c. m.
1.8229	1.8183	1.8196	1.8188

The result shows that in going from a density of 20 centimetres to 200 centimetres per ampere, the amount of copper per coulomb changed from .0003289 to .0003283 for the coils, and for the plates, .0003285 to .0003275.

The result with the plates does not differ materially from that found by Mr. Gray for a similar range.

Mr. Genung also made a number of comparisons of the spiral coil volameter at different current densities, and at a mean temperature of 23 degrees centigrade, with the standard tangent galvanometer at the Magnetic Observatory. They were made with great care in every particular but one, and that was in the matter of time. This was taken from his own watch, that had not been compared with our standard chronometer. Again, to make and break the circuit, a large switch capable of carrying 250 amperes was operated, which added incidental errors, since the duration of each deposit was only 1,800 seconds; the results are interesting, because of their accordance and the manner in which they were obtained, and are given in the following table:

Date.	Size.	Current.	Gain in grammes.		Grammes per ampere per hour.
			Coil No. 1.	Coil No. 2.	
May 8 & 9.	110 c.m.	.6788	.3985	.3977	1.176
" "	" "	1.312	.7699	.7704	1.177
" "	" "	1.883	1.1060	1.1055	1.178
" "	" "	2.446	1.4364	1.4372	1.178
" "	116 "	3.008	1.7686	1.7676	1.179
" 6 & 7.	" "	3 532	2.0755	2.0753	1.179
" "	" "	4.016	2.3639	2.3639	1.180
" "	" "	4.495	2.6386	2.6396	1.176
" "	" "	4.930	2.8932	2.8910	1.176
" "	" "	5 363	3.0750	3.0808	1.178
" "	" "	5.701	3.3437	3.3458	1.176
" 8 & 9.	110 "	6.422	.....	3.7815	1 165*

* Deposit rough copper lost from both coils.

Fortenbaugh and West, also students in the laboratory, making use of a watch belonging to the former, known to the writer to have been keeping correct time, and otherwise precisely the same apparatus that Mr. Genung used, obtained the following result:

Duration	40 m.	
Current	1.814	
Coil No. 1 gained	1.036	Area.
" " 2 "	1.035	90 sq. c.m

From this the number of grammes per ampere per hour is computed to be 1.182. And we know that for the above current density it should be 1.183. The galvanometer at the observatory is subject to a fluctuation in its indications by an amount that almost covers the above, due to variations of horizontal intensity throughout the succession of day and night.

The writer and Mr. E. G. Merritt, Fellow in Physics, Cornell University, hope to make a thorough investigation of the "Spiral Coil Voltameter" in its use with silver and copper shortly.

From the ease with which those inexperienced in the use of the copper voltameter obtain consistent results. it was thought well to recommend its use in the meantime to others.

DISCUSSION.

MR. WESTON:—For some years I had occasion to do a great deal of work with a copper voltameter and I was finally compelled to give it up. I found it to be exceedingly unreliable; and no doubt part of that is due to the fact that the circulation of the electrolyte is not as perfect in some cases as in others. I should expect that the better operation of this voltameter as compared with the plate voltameter was largely due to the fact that the electrolyte circulated much more freely and prevented the formation of secondary compounds and the deposition of them with copper. It is, I know, the general impression that copper generally goes down in an extremely pure condition when precipitated by the electric current. That is not a fact at all. The presence of extremely minute traces of organic matter in an ordinary electric bath will change the whole character of the copper. One-tenth of one per cent. of the weight of the sulphate of copper of ordinary gelatine added to the solution will render the deposit absolutely useless for all practical purposes, so that it is almost as brittle as glass. If the density of the current is kept up where it was, before the addition of the organic matter, great

streaks of white material having a silvery lustre will manifest themselves on the electrodes. I do not think that anything has appeared in print in regard to this, but it certainly is a very interesting fact and I think that the peculiar change in the physical properties of the copper so deposited is due to the formation of a peculiar body which was observed by the eminent French chemist Wurtz a few years ago and which he called hydrite of copper. Therefore I have abandoned the copper voltameter. It requires considerable care not only in the preparation of electrodes to properly secure the accuracy to which Prof. Nichols has shown it is capable of being brought, but it also requires care in the selection of the salt. Silver has been so thoroughly tested as to show that with proper care it is capable of giving excellent results. In the case of silver, if the organic matter could be kept in solution, whether there would be any action or not, I do not know. I know it is true of nickel. The addition of an extremely small percentage of organic matter to a nickel solution renders it incapable of yielding any satisfactory results. That was called to my attention by a case of this kind some years ago. Some nickel platers had a large tank of solution and those tanks were usually made of wood lined with asphalt. Occasionally they would leak, and to save their solution they generally emptied it into barrels. The parties in question put the solution into barrels which had been lined with gelatine. When they put their solution back again, after repairing the tank—it was in a day or so—the solution was ruined. No one could tell what it was due to. It was assumed from the general appearance that somebody had wilfully put something into it to destroy it. It showed the presence of a minute trace of organic matter. That, however, did not throw very much light on the subject, because nickel solutions in those days were frequently used with anodes of muslin which had not been washed, but upon very careful inquiry I found that they had been using these barrels lined with gelatine, I therefore made an experiment by the addition of gelatine to the nickel solution and found exactly the same results. In nickel it is comparatively easy to get rid of it; in copper it is very difficult. In nickel a certain addition of permanganate of potash will enable you to oxidize the organic matter and get rid of it. Therefore in using the copper voltameter it is not only necessary to consider the form of the electrodes, but also the purity of the solution. What other substances, such as albu-

men, would do, I don't know. Gelatine has a very marked effect. The silver voltameter of course, will not retain organic matter if used in the ordinary way. If a silver voltameter is used with care there is not any difficulty from the particles floating away. To avoid that, it is well to take a little freshly precipated oxide of silver which has been carefully washed, and add it to the ordinary nitrate of silver of commerce and you will immediately get your solution into a condition where it yields you quite a nice deposit. It is not very close grained, but it does not show the disposition to float away in small particles. By saturating it with a little oxide of silver, you get rid of that trouble, and it seems to me by far the most reliable voltameter.

THE PRESIDENT:—I am sure that we are very much gratified to hear from Mr. Weston on this subject. It is a subject, I know, to which he has given a very large amount of attention. I do not think there is any one more capable of speaking on deposition generally, both from its practical standpoint and from its use in calibration.

MR. MAILLOUX:—I have had considerable experience with the deposition of copper in a practical way, to the extent of depositing it in very large quantities and at fair rates, and sometimes at very excessive rates, several times faster than is supposed to be possible, and sometimes at very slow rates. I have often had occasion to notice the discrepancy between the theoretical rate of deposit corresponding to a given amount of current, and the practical results; and where the rate of current is excessive, that this difference would be very great. I think, as the result of my observation and experience, that it is possible to get very accurate results with the copper voltameter, but in order to do so I think we should restrict ourselves to certain conditions or take them into consideration. In the first place, I think that the copper used should be absolutely pure. Now, I have found repeatedly in depositing copper industrially for electrotyping or electroplating, that the physical properties of the deposit, its ductility, etc., greatly depended on the character of the metal used as the anode. You take for instance, copper which is itself the result of electrical deposition in electrical refineries, and from that you deposit copper and compare it with an old copper anode such as are supplied to electrotypers, and there is a very marked difference in the character of the deposit, and a considerable variation in the amount of the copper deposited. I think that it

was John T. Sprague, of England, who first pointed out the fact that in depositing solutions there is always a tendency to deposit hydrogen, and it has been my experience that in depositing copper it is only under certain conditions that you are entirely free from depositing a certain amount of hydrogen with the copper. You can only avoid that by having a solution which is sufficiently dense for the amount of current and of proper temperature, and sufficient acidity, and of course, circulated. The circulation is probably one of the most important conditions that determine the theoretical accuracy of the voltameter, and that is perhaps the reason why Mr. Ryan's method gives better results than the other. Under these circumstances it is possible to deposit copper without depositing hydrogen. If hydrogen is evolved it may unite with the copper, but you do not get the weight that you would if it were all copper. That is probably where the main error arises. If the anode and solution are pure, I have found, as Mr. Weston states, that the purity of water has much to do with the accuracy of result; and you must use pure sulphate of copper and pay strict attention to the conditions. Under those circumstances, if the current density is low, the solution not too cold and properly agitated, I have no doubt at all that it can be made as accurate as any other voltametric method.

Mr. Weston:—There is one little remark I would like to make in regard to the addition of acid. Some years ago it used to be customary to use a neutral solution. It was thought more satisfactory, because under those conditions the two electrodes were more uniform. Of late years it has been customary to add a small amount of free acid. and that comes right in line with the remark I made a little while ago as to the probable formation of compounds of copper. In electroplating you invariably find that double salts are used—double sulphate of nickel and ammonia for nickel plating or double chloride; and for silver it is double cyanide of silver and potassium. It was supposed to be due there to the fact that cyanide of silver is insoluble, but that is not entirely the reason for it. The truth is that the nearer you come to a double salt the freer you seem to be from these secondary compounds or bodies precipitated. I think that is a fixed fact. Sulphate of nickel, and sulphate of iron, and those metals that are highly electropositive are readily decomposed by the current at first, but if they are not combined with some other salts they reach a certain condition in which the solution won't

give any more metal. They will give nothing but sub-salts. I have seen deposits from nickel baths, certainly one-eighth of an inch, which were nothing else but sub-salts, and that condition is not brought about immediately but gradually in the solutions. Now, with the electro-negative metals, such as gold and silver, that does not seem to be so marked. In silver we cannot get any indication of it at all in the nitrate, but the metal there does not go down in a condition that suits the practical plater. The copper voltameter, of course, has a decided advantage when we come to measure heavy currents. In nickel salts, iron salts and zinc salts, when the solution is a solution of normally pure salts, the metal will go down, but we all know that these normal salts can be converted into salts that give no acid reaction. By the addition of a certain amount of oxide they form basic salts. Oxide of nickel and oxide of zinc and oxide of iron, freshly precipitated, are all soluble in the normal salts. An addition, therefore, of a very minute trace of oxide to the solutions, so as to make them basic in their character, immediately renders the solution incapable of giving satisfactory results. That condition will be brought about—notwithstanding the fact that you start with a normal salt—particularly where the anode is larger than the cathode. In zinc salts, even when the anode and cathode have the same surfaces, it is usually brought about. Care should be taken in the copper voltameter to obtain a considerable excess of free acid.

Mr. Mailloux:—The currents I measured were seldom below 100 and mostly up to 300 amperes, and I do not think silver will be very useful there.

Dr. Moses:—In corroboration of several statements that were made I would recall the fact that the presence of nitrogen in the cyanide compound has some bearing on changing the physical condition of the copper. There is a very remarkable difference between copper deposited from a cyanide solution and copper deposited from a sulphate of copper solution. They look like two distinct metals. The statement of Mr. Weston is highly interesting and ought to have some bearing on the construction of voltameters. With regard to the discrepancies and difficulties that occur in the use of the sulphate of copper, from the very long and interesting experiments that Prof. Nichols made some ten years ago, I think he ought to be thoroughly satisfied that sulphate of copper is not the right thing.

Mr. E. T. Birdsall:—I am sure every one who has pa-

tience to use the copper voltameter will heartily thank Mr. Ryan if he has made their labors easy. I also think it is possible that this form of the helix of copper wire, if it is as good as the figures appear to indicate, will be a valuable hint to those companies that use electrolytic meters. All electrolytic meters that I know of at present use plates which are somewhat in the form of the old copper voltameter, and although these meters are said to be accurate, yet if they are changed into the form of a helix of copper wire in place of the plates, we shall have perfection perfected.

PROF. NICHOLS:—I would like to say that Mr. Ryan's results have been corroborated throughout the year by student after student. We have no more trouble with the copper voltameter since the adoption of this method. Formerly, as I stated, we expected errors of 1, 2, 3 or 4 per cent., according to the skill of the man, but now we get errors of $\frac{1}{10}$, $\frac{2}{10}$ and $\frac{3}{10}$ per cent. If a man gets to $\frac{3}{10}$ per cent. we tell him something is wrong.

THE PRESIDENT:—The next paper is on "The Personal Error in Photometry," by Prof. Nichols. Prof. Cross's paper on the Measurements of Telephonic Currents will not be read at this meeting, as the Professor is unable to be present.

A Paper read before the American Institute of Electrical Engineers, New York, May 22nd, 1889.

THE PERSONAL ERROR IN PHOTOMETRY.

BY EDWARD L. NICHOLS.

Whenever, in the course of photometric work, different observers have occasion to compare the same pair of lamps, it will be found that the results of their readings with the Bunsen photometer differ by an amount very much larger than the apparent mean error of a single observation.

Ordinarily, these differences are cloaked by the accidental errors due to the fluctuations of the lights under observation, and they have, consequently, not received the attention which they merit. There has been, indeed, so far as I know, no systematic attempt to determine the precise nature and importance of this personal error, the existence of which has doubtless been recognized by very many observers.

The introduction of the storage battery and the adoption of the incandescent lamp, supplied from the constant source thus afforded, as a secondary standard, has, however, entirely removed the large accidental errors resulting from uncontrollable fluctuations in our standard of illumination, and the personal errors just alluded to accordingly stand out in their true importance. I have very recently had occasion to make some experiments upon this subject, the results of which may be of some interest to those who have occasion to work with the Bunsen photometer.

The character of the errors with which one meets continually, whenever the reading of two or more observers are brought into direct comparison, may be very well shown by means of two sets of readings made by Mr. B. W. Snow and myself, upon the same pair of lamps. These observations were made under precisely similar and uncommonly favorable conditions. The two lamps in question were of the same type, and being the only lamps in circuit with a large storage battery, they could be maintained at an almost absolutely constant voltage. These two sets of readings

are not presented here as an example of what may be done with the Bunsen photometer in the comparison of constant sources of light. They are indeed of no great accuracy, and contain larger accidental errors than some observations to which I shall have occasion to call attention presently. They exhibit, however, very clearly the existence of the personal errors of observation which are to form the subject of this paper.

TABLE I.

Two parallel sets of 10 readings upon the candle power of an incandescent lamp.

(1) (B. W. S. observing.)			(2) (E. L. N. observing.)		
Readings.	Differences.		Readings.	Differences.	
C. P.	C. P.	PER CENT.	C. P.	C. P.	PER CENT.
12.12	.112—	.91	12.56	.044—	.35
12.32	.088—	.72	12.60	.084—	.67
12.16	.072—	.59	12.36	.156—	1.25
12.16	.072—	.59	12.56	.044—	.35
12.28	.048—	.39	12.64	.124—	.99
12.40	.168—	1.38	12.60	.048—	.67
12.28	.048—	.39	12.56	.044—	.35
12.08	.152—	1.30	12.40	.116—	.93
12.12	.112—	.92	12 32	.196—	1.35
12.40	.168	1.24	12.56	.044—	.35
AVERAGES.					
12.232	.104	.852	12.516	.089	.717

It will be seen from the above table that while the apparent mean error of a single observation is only about one-tenth of a candle, and the probable error of each set only .022 candle, the two averages themselves differ by .284 candle.

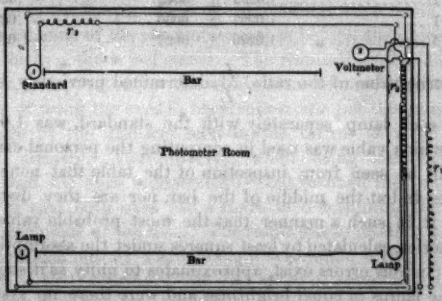
The following experiments were made for the purpose of determining the nature of this personal error:—

Three incandescent lamps of the same type and voltage were selected, care being taken to choose specimens, the carbons of which were as straight as possible. These lamps gave 16 candles at 110 volts. They were connected in multiple to the terminals of a storage battery giving a potential difference of 120 volts. Adjustable resistances of german silver wire were placed in the line leading to each lamp and in one of the mains leading from the battery. (See figure.)

The lamps, together with the above mentioned resistances, were set up in a photometer room which contained two Bunsen photometers. The connecting wires were of such length that the lamps could be moved from one photometer bar to the other without breaking connections. One of the lamps was set up as a comparison standard at the end of the shorter bar. Each of the other

lamps was then compared with this standard, the resistances being adjusted until the intensities of these lamps were found to bear the same relation to the standard. In other words, they were brought to the same intensity by a method which eliminated the systematic personal errors which it was my purpose to study.

The two lamps were then placed at the ends of the longer photometer bar, the length of which was 400 centimeters. It was divided into 800 equal parts. The lamps had been measured with the plane of the filament at right angles with the photometer bar, and care was taken in setting them up in their new position, to place them at the same angle and presenting the same face to the bar as before.



A closed circuit voltmeter between the terminals of the battery enabled the observer at the beginning of a set of readings to bring the lamps to the proper potential. In order to insure greater constancy in the degree of incandescence throughout the series of tests, the lamps were maintained at a potential considerably below the normal, their candle-power being about 12 instead of 16 candles.

A number of observers, all of whom were accustomed to photometric measurements, and some of whom had had extended experience, were asked to make a set of 10 readings each, upon the two lamps. The result of these two readings are given in table II.

Table II.

Ratio of two incandescent lamps, previously adjusted to equal brightness. The measurements are made in the usual manner, by means of the Bunsen photometer.

I_r and I_l are the intensities of the right-hand and left-hand lamps respectively.

Observer.	Ratio—$\frac{I_r}{I_l}$	Personal error.
A	1.0590 ± .0040	— .0558
B	0.9704 ± .0044	+ .0331
C	1.0021 ± .0022	— .0189
D	1.0191 ± .0072	— .0159
E	1.0182 ± .0089	— .0150
F	1.0902 ± .0057	— .0870
G	1.0733 ± .0058	— .0701
H	1.0293 ± .0042	— .0261
I	1.0297 ± .0050	— .0263
J	1.0220 ± .0027	— .0188

The true value of the ratio, $\frac{I_r}{I_l}$ determined previously by comparing each lamp separately with the standard, was 1.0032 ± .0015, which value was used in computing the personal error.

It will be seen from inspection of the table that none of the averages fall at the middle of the bar, nor are they distributed around it in such a manner that the most probable value of the entire series, calculated by least squares under the assumption that only fortuitous errors exist, approximates to unity as it should do, were the lamps of equal brightness and were there no systematic errors to vitiate the result. On the other hand the readings lie, with a single exception, on one side, namely to the left-hand of the centre of the photometer bar. It will be seen, moreover, that the probable error calculated for each set separately, without taking cognizance of any systematic error, is very small in comparison with the differences between the mean results of the various sets, and especially in comparison with the variations of those averages from unity.

The Bunsen disc with which these observations were made, was mounted in the usual manner, the two sides of the disc being viewed simultaneously by means of two plane mirrors set at the proper angle behind the former. It was noted that the almost universal habit in reading was to use the two eyes independently, one eye fixed on each side of the disc. It seemed probable, therefore, that the personal error arose from the unequal sensitive-

ness of the observer's eyes, in that he would unconsciously set the disc at too great a distance from the lamp which illuminated the side of the disc which was being observed by his more sensitive eye. It appears, if we accept this explanation, that the right eye was the more sensitive in all the cases under observation, excepting one, that of the writer (see table II. B), whose readings would indicate the opposite peculiarity.

In order to reverse the relation of the observer to the photometer bar, without altering any of the other conditions of measurement, a large mirror was set up opposite the photometer at a distance of about 40 centimeters. The observer by sitting with his back to the bar, could then see the images of the Bunsen disc in the mirror, and his right and left eyes were reversed with reference to the lamps. Sets of 10 readings were made in this manner by some of the same observers as before, the object being to test the hypothesis just stated. The results thus obtained were, however, complicated by the unforeseen circumstance that the method of using the eyes in observation was not the same when the image of the disc was seen in the mirror as when it was viewed directly. The observer, when using the mirror, no longer used his eyes independently, but scanned the image as a whole, so that both eyes had a share in determining the brightness of each side of the disc. The result was to produce a change in his settings, not by introducing a systematic error, equal and opposite to that occuring in the first series of observations, as had been expected, but by eliminating the error in question. In a word, the settings with the mirror were in better agreement than those made by direct observation, and they gave results much more nearly in accordance with the known equality of the lamps.

In my own case, however, an exception to the above statement must be noted, very possibly as the result of an effort to repeat with the mirror the precise method of observation followed in making the direct settings. I continued to use my eyes independently, viewing the image of one side of the disc, with the right eye, the other with the left. There resulted a set of readings, such as I had expected to obtain from all the observers, the mean of which lay as far from the centre of the photometer bar as the mean of the direct readings, but upon the other side. (Compare sets "B," table II. and III.)

The results obtained with the mirror are given in the following table:—

TABLE III.

Ratio of the two lamps determined by observing the images of the Bunsen disc in a large mirror; the observer placed with his back to the photometer bar:

Observer.	Ratio. $\frac{I_r}{I_l}$	Personal error.
A	1.0070 ± .0026	.0028
B	1.0444 ± .0011	.0414
C	0.9964 ± .0053	.0068
F	1.0141 ± .0025	.0109
G	1.0004 ± .0056	.0028
H	1.0134 ± .0045	.0102
I	1.0004 ± .0031	.0028

The observations with the mirror were followed by four sets of direct readings made with one eye bandaged. It was found that although more fatiguing, these monocular settings were made with a feeling of certainty on the part of the observer which had not accompanied the settings made with both eyes. In the latter case, indeed, a conflict between tendencies to set the disc in two distinct positions had been very apparent, the observer unconsciously choosing now one, now the other, as the true positions. The set of readings given in full in table IV., will serve to illustrate the result of this tendency. The instances in which the observer seems thus to have temporarily changed in his judgment of the true setting, are printed in heavy type.

TABLE IV.

No.	Reading.	No.	Reading.
1	1.033	6	**1.048**
2	1.022	7	**1.053**
3	1.031	8	1.028
4	1.023	9	1.033
5	**1.050**	10	1.030

This tendency to vacillation disappeared almost entirely when only one eye was used. The readings obtained with a single eye not only agreed much better among themselves than those made under like conditions, using both eyes, but the results obtained with the left eye were identical with those obtained with the right eye, and both seemed to be entirely free from the systematic personal error that had been found to vitiate readings made in the usual way.

The results of such a set of monocular readings, made by Mr. Snow and myself, all other conditions remaining the same as in previous trials, are given in table V.

It will be seen that the probable error of each set is much smaller than in the sets of observations made with both eyes; also that the readings of two observers whose mean results from similar sets with both eyes had differed by 8 per cent. (see sets "A" and "B," table II.), are in complete agreement.

Finally, it will be noted that the results of these four monocular series differ from the accepted value of the ratio of intensities of the two lamps (1.0032) by an amount less than the probable error of each set.

TABLE V.

Observer.	Eye used.	Ratio.	Personal error.
B. W. S.	(Right.)	1.0028 ± .0019	.0004
B. W. S.	(Left.)	1.0001 ± .0019	.0031
E. L. N.	(Right.)	1.0001 ± .0017	.0031
E. L. N.	(Left.)	1.0031 ± .0018	.0001

It had been my intention to extend the experiments described in this paper to a much larger number of observers. The evidence obtained, however, seemed quite sufficient to establish the existence of the personal error in photometry, and to show that it was in general very far from being a negligible quantity. Frequent repetitions showed, that in my own case at least, the error was very nearly constant; and I think that it would be quite possible to establish a personal equation, and to apply the proper correction.

A much better plan, however, would be to so modify the photometer car itself as to insure the use of both eyes in the inspection of each side of the illuminated disc. Several forms of photometer, already in use, partially fulfill the necessary requirement. Any device which would bring the images of the opposite faces of the disc into a vertical line in the field of view would doubtless serve to eliminate the error in question. Observations with such a photometer would correspond in character to the "monocular" readings given in table V. Not only would they be free from the systematic error to which the ordinary form of Bunsen photometer is subject, but the accidental errors would be much smaller, and the degree of uncertainty which attends the determination of candle-power by our present methods would be in great measure diminished.

Physical Laboratory of Cornell University, May 17, 1889.

A lecture before the American Institute of Electrical Engineers, at the College of the City of New York, May 22, 1889.

ON MODERN VIEWS WITH RESPECT TO ELECTRIC CURRENTS.

BY PROF. HENRY A. ROWLAND.

As, a short time since, I stood in a library of scientific books and glanced around me at the works of the great masters in physics, my mind wandered back to the time when the apparatus for a complete course of lectures on the subject of electricity consisted of a piece of amber and a few light bodies to be attracted by it. From that time until now, when we stand in a magnificent laboratory with elaborate and costly apparatus in great part devoted to its study, how greatly has the world changed and how our science of electricity has expanded both in theory and practice until, in the one case, it threatens to include within itself nearly the whole of physics, and in the other to make this the age of electricity.

Were I to trace the history of the views of physicists with respect to electric currents it would include the whole history of electricity. The date when the conception of an electric current was possible was when Stephen Gray, about 170 years ago, first divided bodies into conductors and non-conductors, and showed that the first possessed the property of transmitting electrical attractions to a distance. But it was only when the Leyden jar was discovered that the idea of a current became very definite. The notion that electricity was a subtle fluid which could flow along metal wires as water flows along a tube, was then prevalent, and, indeed, remains in force to-day among all except the leaders in scientific thought. It is not my intention to depreciate this notion, which has served and still serves a very important purpose in science. But, for many years, it has been recognized that it includes only a very small portion of the truth and that the mechanism by which energy is transmitted from one

point of space to another by means of an electric current is a very complicated one.

Here, for instance, on the table before me are two rubber tubes filled with water, in one of which the water is in motion, in the other at rest. It is impossible, by any means now known to us, to find out, without moving the tubes, which one has the current of water flowing in it and which has the water at rest. Again, I have here two wires, alike in all respects, except that one has a current of electricity flowing in it and the other has not. But in this case, I have only to bring a magnetic needle near the two to find out in which one the current is flowing. On our ordinary sense the passage of the current has little effect; the air around it does not turn green or the wire change in appearance. But we only have to change our medium from air to one containing magnetic particles to perceive the commotion which the presence of a current may cause. Thus this other wire passes through the air near a large number of small suspended magnets, and, as I pass the current through it, every magnet is affected and tends to turn at right angles to the wire and even to move toward it and wrap itself around it. If we suppose the number of these magnets to become very great and their size small, or if we imagine a medium, every atom of which is a magnet, we see that no wire carrying a current of electricity can pass through it without creating the greatest commotion. Possibly this is a feeble picture of what takes place in a mass of iron near an electric current.

Again, coil the wire around a piece of glass, or indeed, almost any transparent substance, and pass a strong current through the wire. With our naked eye alone we can see no effect whatever, as the glass is apparently unaltered by the presence of the current; but, examined in the proper way, by means of polarized light, we see that the structure of the glass has been altered throughout in a manner which can only be explained by the rotation of something within the glass many millions of times every second.

Once more, bring a wire in which no current exists nearer and nearer to the one carrying the current, and we shall find that its motion in such a neighborhood causes or tends to cause an electric current in it. Or, if we move a large solid mass of metal in the neighborhood of such a current we find a peculiar resistance unfelt before, and if we force it into motion we shall perceive that it becomes warmer and warmer as if there was great friction in moving the metal through space.

Thus, by these tests, we find that the region around an electric current has very peculiar properties which it did not have before, and which, although stronger in the neighborhood of the current, still extend to indefinite distances in all directions, becoming weaker as the distances increase.

How great then the difference between a current of water and a current of electricity. The action of the former is confined to the interior of the tube, while that of the latter extends to great distances on all sides, the whole of space being agitated by the formation of an electric current in any part. To show this agitation, I have here two large frames with coils of wire around them. They hang face to face about 6 feet apart. Through one I discharge this Leyden jar, and immediately you see a spark at a break in the wire of the other coil, and yet there is no apparent connection between the two. I can carry the coils 50 feet or more apart, and, yet, by suitable means I can observe the disturbances due to the current in the first coil.

The question is forced upon us as to how this action takes place. How is it possible to transmit so much power to such a distance across apparently unoccupied space? According to our modern theories of physics there must be some medium engaged in this transmission. We know that it is not the air, because the same effects take place in a vacuum, and, therefore, we must fall back on that medium which transmits light and which we have named the ether. That medium which is supposed to extend unaltered throughout the whole of space, whose existence is very certain but whose properties we have yet but vaguely conceived.

I cannot, in the course of one short hour, give even an idea of the process by which the minds of physicists have been led to this conclusion, or the means by which we have finally completely identified the ether which transmits light with the medium which transmits electrical and magnetic disturbances. The great genius who first identified the two is Maxwell, whose electro-magnetic theory of light is the centre around which much scientific thought is to-day revolving, and which we regard as one of the greatest steps by which we advance nearer to the understanding of matter and its laws. It is this great discovery of Maxwell which allows me, at the present time, to attempt to explain to you the wonderful events which happen everywhere in space when one establishes an electric current in any other portion.

In the first place, we discover that the disturbance does not

take place in all portions of space at once, but proceeds outwards from the centre of the disturbance with a velocity exactly equal to the velocity of light. So that, when I touch these wires together so as to complete the circuit of yonder battery, I start a wave of ethereal disturbance which passes outwards with a velocity of 185,000 miles per second, thus reaching the sun in about eight minutes, and continue to pass onwards forever or until it reaches the bounds of the universe. And, yet, none of our senses inform us of what has taken place unless we sharpen them by the use of suitable instruments. Thus, in the case of these two coils of wire, suspended near each other, which we have already used, when the wave from the primary disturbance reaches the second coil, we perceive the disturbance by means of the spark formed at the break in the coil. Should I move the coils further apart, the spark in the second coil would be somewhat delayed, but the distance of 185,000 miles would be necessary before this delay could amount to as much as one second. Hence the effects we observe on the earth take place so nearly instantaneously that the interval of time is very difficult to measure, amounting, in the present case, to only $\frac{1}{100,000,000}$ of a second.

It is impossible for me to prove the existence of this interval, but I can at least show you that waves have something to do with the action here observed. For instance, I have here two tuning forks mounted on sounding boxes and tuned to exact unison. I sound one and then stop its vibrations with my hand, instantly you hear that the other is in vibration, caused by the waves of sound in the air between the two. When, however, I destroy the unison by fixing this piece of wax on one of the forks, the action ceases.

Now, this combination of a coil of wire and a Leyden jar is a vibrating system for electricity and its time of vibration is about 10,000,000 times a second. This second system is the same as the first, and therefore its time of vibration is the same. You see how well the experiment works now because the two are in unison. But let me take away this second Leyden jar, thus destroying the unison, and you see that the sparks instantly cease. Replacing it, the sparks reappear. Adding another on one side and they disappear again, only to reappear when the system is made symmetrical by placing two on each side.

This experiment and that of the tuning forks have an exact analogy to one another. In each we have two vibrating systems

connected by a medium capable of transmitting vibrations, and they both come under the head of what we know as sympathetic vibrations. In the one case, we have two mechanical tuning forks connected by the air; in the other, two pieces of apparatus which we might call electrical tuning forks, connected by the luminiferous ether. The vibrations in one case can be seen by the eye or heard by the ear, but in the other case they can only be perceived when we destroy them by making them produce a spark. The fact that we are able to increase the effect by proper tuning demonstrates that vibrations are concerned in the phenomenon. This can, however, be separately demonstrated by examining the spark by means of a revolving mirror, when we find that it is made up of many successive sparks corresponding to the successive backward and forward movements of the current.

The fact of the oscillatory character of the Leyden jar discharge was first demonstrated by our own countryman, Henry, in 1832, but he pursued the subject only a short distance, and it remained for Sir William Thomson to give the mathematical theory and prove the laws according to which the phenomenon takes place.

Thus, in the case of a charged Leyden jar whose inner and outer coatings have been suddenly joined by a wire, the electricity flows back and forth along the wire until all the energy originally stored up in the jar has expended itself in heating the wire or the air where the spark takes place and in generating waves of disturbance in the ether which move outward into space with the velocity of light. These ethereal waves we have demonstrated by letting them fall on this coil of wire and causing the electrical disturbance to manifest itself by electric sparks.

I have here another more powerful arrangement for producing electro-magnetic waves of very long wave length, each one being about 500 miles long. It consists of a coil, within which is a bundle of iron wires. On passing a powerful alternating current through the coil, the iron wires are rapidly magnetized and demagnetized and send forth into space a system of electro-magnetic waves at the rate of 360 in a second.

Here, also, I have another piece of apparatus [a lamp] for sending out the same kind of electro-magnetic waves; on applying a match, we start it into action. But the last apparatus is tuned to so high a pitch that the waves are only $\frac{1}{10,000}$ inch long, and 55,000,000,000,000 are given out in one second. These short waves are

known by the name of light and radiant heat, though the name radiation is more exact. Placing any body near the lamp so that the radiation can fall on it, we observe that when the body absorbs the rays it is heated by them; the well-known property of so-called radiant heat and light. Is it not possible for us to get some substance to absorb the *long* waves of disturbance, and so obtain a heating effect? I have here such a substance in the shape of a sheet of copper, which I fasten on the face of a thermopile, and I hold it where the waves are the strongest [near the coil while the alternating current is passing through it]. As I have anticipated, great heat is generated by their absorption, and soon the plate of copper becomes very warm, as we see by this thermometer, by feeling it with the hand or even by the steam from water thrown upon it. In this experiment the copper has not touched the coil or the iron wire core, although if it did they are very much cooler than itself. The heat has been produced by the absorption of the waves in the same way as a blackened body absorbs the rays of shorter wave length from the lamp; and, in both cases, heat is the result.[1]

But in this experiment, as in the first one, the wave-like nature of the disturbance has not been proved experimentally. We have caused electric sparks, and have heated the copper plate across an interval of space, but have not in either of these cases proved experimentally the progressive nature of the disturbance; for a ready means of experimenting on the waves, obtaining their wave length and showing their interferences, has hitherto been wanting. This deficiency has been recently overcome by Prof. Hertz, of Carlsruhe, who has made a study of the action of the coil, and has shown us how to use it for experiments on the ethereal waves, whose existence had before been made certain by the mathematics of Maxwell.

I scarcely know how to present this subject to a non-technical audience and make it clear how a coil of wire with a break in it can be used to measure the velocity and wave length of ethereal waves. However, I can but try. If the waves moved very slowly, we could readily measure the time the first coil took to affect the second, and show that this time was longer as the distance was greater. But it is absolutely inappreciable by any of our instruments, and another method must be found. To obtain the

1. The thermopile was connected with a delicate mirror galvanometer, the deflections of which were shown on a screen.

wave length Prof. Hertz used several methods, but that by the formation of stationary waves is the most easily grasped. Mr. Ames holds in his hand one end of a spiral spring, which makes a very heavy and flexible rope. As he sends a wave down it, you see that it is reflected at the further end, and returns again to his hand. If, however, he sends a succession of waves down the rope, the reflected waves interfere with the direct ones, and divide the rope into a succession of nodes and loops, which you now observe. So a series of sound waves, striking on a wall, form a system of stationary waves in front of the wall. With this in view, Prof. Hertz established his apparatus in front of a reflecting wall, and observed the nodes and loops by the sparks produced in a ring of wire. It is impossible for me to repeat this experiment before you, as it is a very delicate one, and the sparks produced are almost microscopic. Indeed, I should have to erect an entirely different apparatus, as the waves from the one before me are nearly ½ mile long, the time of vibration of the system being very great, that is, $\frac{1}{10,000}$ of a second. To produce shorter waves we must use apparatus tuned, as it were, to a higher pitch, in which the same principle is, however, employed, but the ethereal waves are shorter, and thus several stationary waves can be contained in one room.

The testing coil is then moved to different portions of the room, and the nodes are indicated by the disappearance of the sparks, and the loops by the greater brightness of them. The presence of stationary waves is thus proved, and their half wave length found from the distance from node to node, for stationary waves can always be considered as produced by the interference of two progressive waves advancing in opposite directions.

However interesting a further description of Professor Hertz's experiments may be, we have gone as far in that direction as our subject carries us; for we have demonstrated that the production of a current in a wire is accompanied by a disturbance in the surrounding space; and, although I have not experimentally demonstrated the ethereal waves, yet I have proved the existence of electric oscillations in the coils of wire and the ether surrounding it.

Our mathematics has demonstrated, and experiments like those of Professor Hertz have confirmed the demonstration, that the wave disturbance in the ether is an actual fact.

The closing of a battery circuit, then, and the establishment of

a current of electricity in a wire is a very different process from the formation of a current of water in a pipe, though, after the first shock, the laws of the flow of the two are very much alike. But even then, the medium around the current of electricity has very strange properties, showing that it is accompanied by a disturbance throughout space. The wire is but the core of the disturbance, which latter extends indefinitely in all directions.

One of the strangest things about it is that we can calculate with perfect exactness the velocity of the wave propagation and the amount of the disturbance at every point and at any instant of time; but as yet we cannot conceive of the details of the mechanism which is concerned in the propagation of an electric current. In this respect our subject resembles all other branches of physics in the partial knowledge we have of it. We know that light is the undulation of the luminiferous ether, and yet the constitution of the latter is unknown. We know that the atoms of matter can vibrate with purer tones than the most perfect piano, and yet we cannot even conceive of their constitution. We know that the sun attracts the planets with a force whose law is known, and yet we fail to picture to ourselves the process by which it takes our earth within its grasp at the distance of many millions of miles and prevents it from departing forever from its life-giving rays. Science is full of this half knowledge, and the proper attitude of the mind is one of resignation toward that which it is impossible for us to know at present and of earnest striving to help in the advance of our science, which shall finally allow us to answer all these questions.

The electric current is an unsolved mystery, but we have made a very great advance in understanding it when we know that we must look outside of the wire at the disturbance in the medium before we can understand it: a view which Faraday dimly held fifty years ago, which was given in detail in the great work of Maxwell, published sixteen years since, and has been the guide to most of the work done in electricity for a very long time. A view which has wrought the greatest changes in the ideas which we have conceived with respect to all electrical phenomena.

So far, we have considered the case of alternating electric current in a wire connecting the inner and outer coatings of a Leyden jar. The invention of the telephone, by which sound is carried from one point to another by means of electrical waves, has forced into prominence the subject of these waves. Further-

more, the use of alternating currents for electric lighting brings into play the same phenomenon. Here, again, the difference between a current of water and a current of electricity is very marked. A sound wave, traversing the water in the tube, produces a to and fro current of water at any given point. So, in the electrical vibration along a wire, the electricity moves to and fro along it in a manner somewhat similar to the water but with this difference:—the disturbance from the water motion is confined to the tube and the oscillation of the water is greatest in the centre of the tube, while, in the case of the electric current, the ether around the wire is disturbed and the oscillation of the current is greatest at the surface of the wire and least in its centre. The oscillations in the water take place in the tube without reference to the matter outside the tube, whereas the electric oscillations in the wire are entirely dependent on the surrounding space, and the velocity of the propagation is nearly independent of the nature of the wire, provided only that it is a good conductor.

We have then, in the case of electrical waves along a wire, a disturbance outside the wire and a current within it, and the equations of Maxwell allow us to calculate these with perfect accuracy and give all the laws with respect to them.

We thus find that the velocity of propagation of the waves along a wire, hung far away from other bodies and made of good conducting material, is that of light, or 185,000 miles per second; but when it is hung near any conducting matter, like the earth, or inclosed in a cable and sunk into the sea, the velocity becomes much less. When hung in space, away from other bodies, it forms, as it were, the core of a system of waves in the ether, the amplitude of the disturbance becoming less and less as we move away from the wire. But the most curious fact is that the electric current penetrates only a short distance into the wire, being mostly confined to the surface, especially where the number of oscillations per second is very great.

The electrical waves at the surface of a conductor are thus, in some respects, very similar to the waves on the surface of the water. The greatest motion in the latter case is at the surface while it diminishes as we pass downward and soon becomes inappreciable. Furthermore, the depth to which the disturbance penetrates into the water increases with increase of the length of the wave, being confined to very near the surface for very short

waves. So the disturbance in the copper penetrates deeper as the waves and the time of oscillation are longer, and the disturbance is more nearly confined to the surface as the waves become shorter. I have recently made the complete calculations with respect to these waves, and have drawn some diagrams to illustrate the penetration of the alternating current into metal cylinders. The first diagram represents the current at different depths in a copper cylinder, 45 cm. diameter, or an iron one 14½ cm. diameter, traversed by an alternating current with 200 reversals per second. The first and second curves show us the current at two different instants of time, and show us how the phase

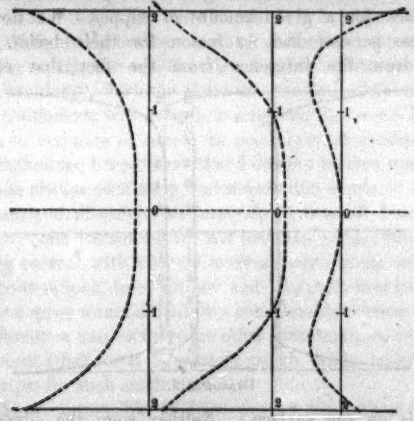


DIAGRAM 1.

changes as we pass downward into the cylinder. By reference to the third curve we see that it may be even in the opposite direction in the centre of the cylinder from what it is at the surface. The third curve gives us the amplitude of the current oscillations at different depths, irrespective of the phase, and it shows us that the current at the centre is only about 10 per cent. of that at the surface in this case. The second diagram shows us the distribution in the same cylinders when the number of reversals of the current is increased to 1,800 per second. Here we see that the disturbance is almost entirely confined to the surface, for at a depth of only 7 mm., the disturbance almost entirely vanishes.

There are very many practical applications of these theoretical results for electric currents. The most obvious one is to the case of conductors for the alternating currents used in producing the electric light. We find that when these are larger than about half an inch diameter they should be replaced by a number of conductors less than half an inch diameter, or by strips about a quarter of an inch thick, and of any convenient width. But this is a matter to be attended to by the electric light companies.

Prof. Oliver J. Lodge has recently, in the British Association, drawn attention to the application of these results to lightning rods. Almost since the time of Franklin, there have been those who advocated the making of lightning rods hollow, to increase the surface for a given amount of copper. We now know that these persons had no reason for their belief, as they simply drew the inference from the fact that electricity

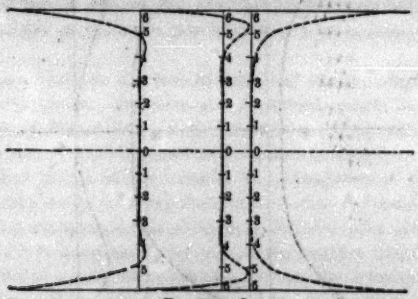


Diagram 2.

at best is on the surface. Neither were the advocates of the solid rods quite correct, for they reasoned from the fact that electricity in a state of steady flow, occupies the whole area of the conductor equally. The true theory, we now know, indicates that neither party was entirely correct and that the surface is a very important factor in the case of a current of electricity so sudden as that from a lightning discharge. But increase of surface can best be obtained by multiplying the number of conductors, rather than making them flat or hollow; and, at the same time, Maxwell's principle of enclosing the building within a cage can be carried out. Theory indicates that the current penetrates only one-tenth the distance into iron that it does into copper. As the iron has seven times the resistance of copper, we

should need 70 times the surface of iron that we should of copper. Hence I prefer copper wire about a quarter of an inch diameter and nailed directly to the house without insulators, and passing down the four corners, around the eaves and over the roof, for giving protection from lightning in all cases where a metal roof and metal down spouts do not accomplish the same purpose.

Whether the discharge of lightning is oscillatory or not, does not enter into the question, provided it is only sufficiently sudden. I have recently solved the mathematical problem of the electric oscillations along a perfectly conducting wire joining two infinite and perfectly conducting planes parallel to each other, and find that there is no definite time of oscillation, but that the system is capable of vibrating in any time in which it is originally started. The case of lightning between a cloud of limited extent and the earth along a path through the air of great resistance is a very different problem. Both the cloud and the path of the electricity are poor conductors, which tends to lengthen the time. If I were called on to estimate as nearly as possible what took place in a flash of lightning, I would say that I did not believe that the discharge was always oscillating, but more often consisted of one or more streams of electricity at intervals of a small fraction of a second, each one continuing for not less than $\frac{1}{100,000}$ second. An oscillating current with 100,000 reversals per second would penetrate about $\frac{1}{60}$ inch into copper and $\frac{1}{160}$ inch into iron. The depth for copper would constitute a considerable portion of a wire $\frac{1}{4}$ inch diameter; and, as there are other considerations to be taken into account, I believe it is scarcely worth while making tubes, or flat strips, for such small sizes.

It is almost impossible to draw proper conclusions from experiments on this subject in the laboratory such as those of Professor Oliver J. Lodge. The time of oscillation of the current in most pieces of laboratory apparatus is so very small, being often the $\frac{1}{100,000,000}$ of a second, that entirely wrong inferences may be drawn from them. As the size of the apparatus increases, the time of oscillation increases in the same proportion, and changes the whole aspect of the case. I have given $\frac{1}{100,000}$th of a second as the shortest time a lightning flash could probably occupy. I strongly suspect it is often much greater, and thus departs even further from the laboratory experiments of Professor Lodge, who has, however, done very much toward drawing attention to this matter and showing the importance of surface in this case. All

shapes of the rod with equal surface are not, however, equally efficient. Thus, the inside surface of a tube does not count at all. Neither do the corrugations on a rod count for the full value of the surface they expose, for the current is not distributed uniformly over the surface; but I have recently proved that rapidly alternating currents are distributed over the surface of very good conductors in the same manner as electricity at rest would be distributed over them, so that the exterior angles and corners possess much more than their share of the current, and corrugations on the wire concentrate the current on the outer angles and diminish it in the hollows. Even a flat strip has more current on the edges than in the centre.

For these reasons, shape, as well as extent of surface, must be taken into account, and strips have not always an advantage over wires for quick discharges.

The fact that the lightning rod is not melted on being struck by lightning is not now considered as any proof that it has done its work properly. It must, as it were, seize upon the discharge, and offer it a easier passage to the earth than any other. Such sudden currents of electricity we have seen to obey very different laws from continuous ones, and their tendency to stick to a conductor and not fly off to other objects depends not only on having them of small resistance, but also on having what we call the self-induction as small as possible. This latter can be diminished by having the lightning rod spread sideways as much as possible, either by rolling it into strips, or better, by making a network of rods over the roof, with several connections to the earth at the corners, as I have before described.

Thus we see that the theory of lightning rods, which appeared so simple in the time of Franklin, is, to-day, a very complicated one, and requires for its solution a very complete knowledge of the dynamics of electric currents. In the light of our present knowledge the frequent failure of the old system of rods is no mystery, for I doubt if there are a hundred buildings in the country properly protected from lightning. With our modern advances, perfect protection might be guaranteed in all cases, if expense were no object.

So much for the rod itself, and now let us turn to other portions of the electrical system, for we have seen that, in any case, the conductor is only the core of a disturbance which extends to great distances on all sides. Were the clouds, the earth and the

streak of heated air called the lightning flash all perfect conductors we could calculate the entire disturbance. It might then consist of a series of stationary waves between the two planes, extending indefinitely on all sides but with gradually decreasing amplitude as we pass away from the centre. The oscillation, once set up, would go on forever, as there would be no poor conductors to damp them. But when the clouds and the path of the lightning both have very great resistance, the energy is very soon converted into heat and the oscillations destroyed. I have given it as my opinion that this is generally the case and that the oscillations seldom take place, but I may be wrong, as there is little to guide me except guess work. If they take place, however, we have a ready explanation of what is sometimes called a back stroke of lightning. That is, a man at the other end of the cloud a mile or more distant from the lightning stroke sometimes receives a shock, or a new lightning flash may form at that point and kill him. This may be caused, according to our present theory, by the arrival of the waves of electrical disturbance which might themselves cause a slight shock or even overturn the equilibrium then existing and cause a new electric discharge.

We have now considered the case of oscillations of electricity in a few instances and can turn to that of steady currents. The closing of an electric current sends ethereal waves throughout space, but that after the first shock the current flows steadily without producing any more waves. However, the properties of the space around the wire have been permanently altered, as we have already seen. Let us now study these properties more in detail. I have before me a wire in which I can produce a powerful current of electricity and we have seen that the space around it has been so altered that a delicately suspended magnetic needle cannot remain quiet in all positions but stretches itself at right angles to the wire, the north pole tending to revolve around it in one direction and the south pole in the other. This is a very old experiment, but we now regard it as evidence that the properties of the space around the wire have been altered rather than that the wire acts on the magnet from a distance.

Put, now, a plate of glass around the wire, the latter being vertical and the former with its plane horizontal, and pass a powerful current through the wire. On now sprinkling iron filings on the plate, they arrange themselves in circles around the wire and thus point out to us the celebrated lines of magnetic force of

Faraday. Using two wires with currents in the same direction we get these other curves, and, testing the forces acting on the wire, we find that they are trying to move towards each other.

Again, pass the currents in the opposite directions and we get these other curves and the currents repel each other. If we assume that the lines of force are like rubber bands, which tend to shorten in the direction of their length and repel each other sideways, Faraday and Maxwell have shown that all magnetic attraction and repulsions are explained. The property which the presence of the electric current has conferred on the luminiferous ether is then one by which it tends to shorten in one direction and spread out in the other two directions.

We have thus done away with action at a distance, and have accounted for magnetic attraction by a change in the intervening medium as Faraday partly did almost fifty years ago. For this change in the surrounding medium is as much a part of the electric current as anything that goes on within the wire.

To illustrate this tension along the lines of force, I have constructed this model which represents the section of a coil of wire with a bar of iron within it. The rubber bands represent the lines of force which pass around the coil and through the iron bar, as they have an easier passage through the iron than the air. As we draw the bar down and let it go, you see that it is drawn upward and oscillates around its position of equilibrium until friction brings it to rest. Here, again, I have a coil of wire with an iron bar within it with one end resting on the floor. As we pass the current and the lines of magnetic force form around the coil and pass through the iron, it is lifted upwards although it weighs 24 pounds and oscillates around its position of equilibrium exactly the same as though it were sustained by rubber bands as in the model. The rubber bands in this case are invisible to our eye but our mental vision pictures them to us as lines of magnetic force in the luminiferous ether drawing the bar upward by their contractile force. This contractile force is no small quantity, as it may amount, in some cases, to one or even two hundred pounds to the square inch, and thus rivals the greatest pressure which we use in our steam engines.

Thus the luminiferous ether is, to-day, a much more important factor in science than the air we breathe. We are constantly surrounded by the two, and the presence of the air is manifest to us all; we feel it, we hear by its aid and we even see it, under

favorable circumstances, and the velocity of its motion as well as the amount of moisture it carries, is a constant topic of conversation with mankind at large. The luminiferous ether, on the other hand, eludes all our senses and it is only with imagination, the eye of the mind, that its presence can be perceived. By its aid in conveying the vibrations we call light, we are enabled to see the world around us, and by its other motions which cause magnetism, the mariner steers his ship through the darkest night when the heavenly bodies are hid from view. When we speak in a telephone, the vibrations of the voice are carried forward to the distant point by waves in the luminiferous ether, there again to be resolved into the sound waves of the air. When we use the electric light to illuminate our streets, it is the luminiferous ether which conveys the energy along the wires as well as transmits it to our eye after it has assumed the form of light. We step upon an electric street car and feel it driven forward with the power of many horses, and again it is the luminiferous ether, whose immense force we have brought under our control and made to serve our purpose. No longer a feeble uncertain sort of medium, but a mighty power, extending throughout all space and binding the whole universe together, so that it becomes a living unit in which no one portion can be changed without ultimately involving every other portion.

To this, ladies and gentlemen, we have been led by the study of electrical phenomena, and the ideas which I have set forth constitute the most modern views held by physicists with respect to electric currents.

—INDEX—

OF

CURRENT ELECTRICAL LITERATURE,

&c. (March, 1889.)

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Chinese Telegraphs. (Note.) *Lond. Elec. Rev.*, March 22, p. 336.

The Post Office Telegraph Service. (Note.) *Lond. Elec. Rev.*, March 22, p. 335.

Work on Indian Telegraphs. *Lond. Elec. Rev.*, March 22, p. 333.

The Indian Telegraph Department. *Indian Engineer; Lond. Elecn.*, March 22, p. 567.

Problems and Conditions of Ocean Telegraphy. (Corr.) R. L. Weatherbe. *Lond. Elec. Rev.*, March 22, p. 346. Samuel Trott. March 29, p. 374.

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Cable Repairing under Difficulties. *Lond. Elec. Engr.*, March 8, p. 192.

The Submarine Company's Cable. (Note.) *Lond. Elecn.*, March 22, p. 559.

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Iron or Hemp Covered Cables. (Ed.) *Elec. World*, March 2, p. 113.

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The Denio Fire Alarm. (Ill.) *Science*, March 15, p. 191; *Lond. Elec. Rev.*, March 29, p. 359.

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The Use of the Telephone on Railways. *Indust*, March 22, p. 284

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The Phonograph Toy Manufacturing Company. *Md. Lt. & Ht.*, March 21, p. 372.

The Bell Patents Sustained Again. *N. Y. Elec. Rev.*, March 2, p. 4

ELECTRIC LIGHT.

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A New Application for Glow Lamps. *Indust.*, March 1, p. 212.

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Electric Lighting by Gas Companies. *Progressive Age; Elec. World*, March 30, p. 183.

Some Figures on Electric Train Lighting. *Elec. World*, March 16, p. 163.

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Efficiency of Glow Lamps with Direct and Alternating Currents. *N. Y. Elec. Rev.*, March 30, p. 1.

Details of Incandescent Lighting. (Ill.) *N. Y. Elec. Rev.*, March 9, p. 10.

Lighting at the Inauguration Ball. *Elec. World*, March 23, p. 176.

The Edison Central Station at Easton, Pa. (Ill.) *Elec. World*, March 16, p. 157.

Electric Train Lighting on the Atchison, Topeka & Santa Fe Railroad. (Ill.) *West. Elecn.*, March 9, p. 139. Train Lighting, March 9, p. 141.

St. Louis Electric Light Contract. *West. Elecn.*, March 9, p. 136.

The Burlington, Vt., Electric Light Plant. *N. Y. Elec. Rev.*, March 23, p. 8.

A Very Strong and Business-like Organization. *N. Y. Elec. Rev*, March 9, p. 5. (Westinghouse.)

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The Metropolitan Electric Stations. (Note.) *Lond. Elec. Engr.*, March 22, p. 229.

The British Museum. *Lond. Elec. Rev.*, March 29 p. 365.

The Electric Light and the Paris Exhibition. *Lond. Elec. Rev*, March 29, p. 352.

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Testing Chain Cables by Electric Light. (Note.) *Lond. Elecn.*, March, p. 476.

Electric Lighting at the Erection of the Forth Bridge. Alfred E. Mavor. *Lond. Elecn.*, March 29, p. 586.

Progress of Electric Installations in London. *Sci. Am*, March 30, p. 196.

Electric Lighting in Dieulefit and Valreas. (Ill.) M. Hospitalier. *La Nature; Elec. World*, March 16, p. 159.

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Paris Palais Royal Central Station. (Ill.) *Électricité; West. Elecn.*, March 23, p. 159.

A Large Electric Light Plant at St. Louis Electrical Exhibition. *N. Y. Elec. Rev.*, March 30, p. 7; *West. Elecn.*, March 23, p. 160; *Elec. World*, March 23, p. 176.

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Electric Light Circuits. (Corr.) *Lond. Elec. Engr.*, March 29, p. 251.

Standard of Insulation for Electric Light Circuits. G. E. Fish. *West. Elecn.; Lond. Elec. Engr.*, March 22, p. 232.

On the Place of Accumulators in Electric Lighting. G. B. Prescott, Jr. *N. Y. Elec. Engr.*, March, p. 84.

Municipal Control of Commercial Lighting. (A. D. Chandler before the Mass. Legislature.) *Md. Lt. & Ht.*, March 21, p. 374.

The Chicago Convention. *N. Y. Elec. Rev.*, March 9, p. 4.

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Ninth Meeting of the National Electric Light Association. *Elec. World*, March 2, p. 114; *N. Y. Elec. Engr.*, March, p. 138; *Elec. Pr.*, March, p. 72; *Lond. Elec. Rev.*, March 8, p. 270; *Lond. Elec. Engr.*, March 15, p. 209; March 22, p. 235; *N. Y. Elec. Rev.*, March 2, pp. 1–8; *Science*, March 1, p. 154; *Md. Lt. & Ht.*, March 7, p. 317; *Sci. Am.*, March 9, p. 144.

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Central Station Lighting. Prof. Geo. Forbes. *N. Y. Elec Rev.*, March 30, p. 8.

Proposed Amendment of the Pennsylvania Statutes Pertaining to Electric Light, etc. *Md. Lt. & Ht.*, March 21. p. 370.

The Edison Incandescent Lamp Patents in England. *West. Elecn.*, March 16, p. 149.

Edison-Westinghouse Litigation. *West. Elecn.*, March 2, p. 130.

Voiding Edison Patents in Canada. *Elec. World*, March 16, p. 162; March 23, p. 177; *N. Y. Elec. Engr.*, March, p. 153; *N. Y. Elec. Rev.*, March 23, p. 2; *West. Elecn*, March 16, p. 147; March 23, p. 166; *Md. Lt. & Ht.*, March 21, p. 380.

The Efficiency of Methods of Artificial Illumination. (Ill.) E. L. Nichols. *West. Elecn.*, March 30, p. 177.

Vienna Soft Cored Carbons. *Elec. World*, March 16, p. 161.

DYNAMOS, SYSTEMS OF DISTRIBUTION, Etc.

A New Dynamo. (Fritsche & Pischon.) *Science*, March 29, p. 236.

The Fritsche Wheel Dynamo. (Ill.) *Indust.*, March 15, p. 248.

Wheel Armature Dynamo. (Ill.) *West. Elecn.*, March 30, p. 172.

The Sperry Regulator. (Ill.) *West. Elecn.*, March 9, p. 135.

On the Designing of Continuous Current Dynamo Machines. (Ill.) Prof. Jamieson. *Lond. Elec. Rev.*, March 8, p. 263; *Lond. Elec. Engr.*, March 1, p. 180; March 8, p. 196; March 29, p. 256; *Lond. Elecn.*, March 1, p. 487; March 8, p. 515; March 15, p. 549.

A Synthetic Study of Dynamo Machines. (Ill.) (Cont'd from p. 179.) *Lond. Elec. Rev.*, March 1, p. 237; March 15, p. 292.

A Pointer on Dynamo Mishaps. W. F. G. *Elec. World*, March 30, p. 184.

The Slow Baking of Dynamo Armatures. R. H. Sterling. *Elec. World*, March 16, p. 157.

Thomson's New Welding Transformer and Dynamo. (Ill.) *Elec. World*, March 23, p. 173.

Novel Transformer. (Ill.) *West. Elecn.*, March 2, p. 125. (F. B. Badt.)

Alternate Current Machinery. Gilbert Kapp. Inst. Civ. Engrs. *Lond. Elec. Rev.*, March 1, p. 253; *Indust.*, March 1, p. 211; *Elec. World*, March 9, p. 151.

Patents on Alternating Current Transformers. (Gaulard & Gibbs.) *Science*, March 29, p. 235; *Indust.*, March 8, p. 230; *Lond. Elec. Rev.*, March 8, pp. 274-282.

The Adverse Gaulard-Gibbs Decision in England Sustained. *Elec. World*, March 9, p. 141.

Gans & Co's Continuous Current System. (Ill.) *West. Elecn.*, March 2, p. 124.

Distribution by Transformers. (Ed.) *Lond. Elec. Engr.*, March 8, p. 194.

The Zipernowski-Deri Alternate Current System. (Ill.) F. B. Badt, *Lond. Elec. Engr.*, March 1, p. 178.

System of Electrical Distribution. (G. Westinghouse.) *West. Elecn.*, March 23, p. 160.

Electric Distribution in Paris. (Ill.) *Elec. World*, March 16, p. 156.

Compound Winding Patents. King, Brown & Co. vs. Anglo-Am. Brush E. L. Corporation. (Cont'd from p. 740.) *Lond. Elec. Rev.*, March 1, p. 249; March 8, p. 283; March 15, p. 312.

ELECTRIC MOTORS.

The Connecticut Motor. (Ill.) *Elec. World*, March 30, p. 189.

The Connecticut Motor Company. (Ill.) *N. Y. Elec. Rev.*, March 30. p. 1.

The Denison Motor and Dynamo. (Ill.) *Science*, March 22, p. 214.

Alternate Current Motors. (Ed.) *Lond. Elec. Rev.*, March 8, p 258.

The Tesla Motor. (Ill.) *Lond. Elecn.*, March 1, p. 480.

The Tesla Alternating Current Motor. *Science*, March 29, p. 235.

The Electric Motor in Factories. (C. & C.) (Ill.) *Science*, March 1, p. 153.

A Suggestion in Canal Boat Propulsion. (Ill.) *Lond. Elec. Rev.*, March 29, p. 357.

The Transmission of Power by Electricity. (Ill.) Frank J. Sprague Fkln. Inst. *Lond. Elec. Rev.*, March 22, p. 326; March 29, p 355.

Cheap Motors for Small Powers. *Lond. Elec. Rev.*, March 8, p. 276.

An Electric Motor in Competition with Steam. *Elec. World; Lond. Elec. Rev.*, March 8, p. 269.

The Electric Motor in Engineering. (Note.) *Lond. Elec. Engr.*, March 29, p. 248.

Electric Drawbridge. (Note.) *Lond. Elec. Engr.*, March 22, p. 226.

Electric Tree Felling Machine. (Ill.) *Sci. Am. Supp.*, March 2, p. 10,970.

Sperry's Electrical Coal Digger. (Ill.) *Elec. World*, March 23, p. 173.

The Submarine Boat "Gymnote." *Science*, March 22, p. 212.

Something Electricity is Doing. C. Barnard. *Century; West. Elecn.*, March 9, p. 142; March 16, p. 153.

ELECTRIC RAILWAYS, ETC.

Recent Sprague Roads. (Ill.) Hartford, Conn., Davenport, Ia., and Scranton, Pa. *West. Elecn.*, March 16, p. 148; March 23, p. 160; March 30, p. 172.

Sprague Electric Railway at Cleveland, O. (Ill.) *West. Elecn.*, March 2, p. 129.

The Scranton, Pa., Electric Power Station. (Ill.) *Elec. World*, March 16, p. 161.

Electric Traction in America. Sprague System at Brockton, Reading and Wilmington. (Ill) *Lond. Elec. Engr.*, March 1, p. 170.

Street Railways to be Equipped with Electric Motors. *Elec. World*, March 30, p. 194.

Electric Railway at St. Joseph, Mo. (Ill.) *N. Y. Elec. Rev.*, March 30, p. 10.

The Bentley-Knight Electric Railway Company. (Ill.) *N. Y. Elec. Rev.*, March 2, p. 3.

The West End Electric Railway. (Ill.) *N. Y. Elec. Rev.*, March 9, p. 17.

The Thomson-Houston Electric Railway at Des Moines. (Ill.) *Elec. World*, March 9, p. 145; *West. Elecn.*, March 9, p. 138.

Sprague Electric Roads at Wilkesbarre, Pa., Steubenville, O., and Asheville, N. C. *N. Y. Elec. Rev.*, March 23, p. 9; *Science*, March 8, p. 174; March 22, p. 212. (Ill.) *Elec. World*, March 23, p. 175; St. Joseph, Mo., March 30, p. 188; Boston, Mass., Hartford, Conn., and Davenvenport, Ia., March 16, p. 160.

The Thomson-Houston Electric Road at Syracuse, N. Y. (Ill.) *Elec. World*, March 23, p. 174; *N. Y. Elec. Rev.*, March 23, p. 3; *Md. Lt. & Ht.*, March 14, p. 345; Cambridge, Mass., Wichita, Kan., March 9, p. 145.

The Gravesend Electric Tramway. Series System. (Ill.) *Lond. Elecn.*, March 15, p. 541.

The Series Electrical Tramway at Northfleet. (Ill.) *Lond. Elec. Engr.*, March 15, p. 215; *Indust.*, March 15, p. 258.

Series Electrical Traction for Tramlines. (Ill.) *Lond. Elec. Rev.*, March 15, p. 295.

The Robinson-Foster System of Electric Railways and Motors. (Ill.) *Md. Lt. & Ht.*, March 28, p. 400.

The Riley Elevated Electric Railway System. (Ill.) *Elec. World*, March 16, p. 162.

A Large Electric Current Conductor. (Ill.) *Science*, March 29, p. 230. (Daft conductor, N. Y. Elevated Road.)

Electric Propulsion on the New York Elevated Railways. (Ill.) *Elec. World*, March 23, p. 172; *Sci. Am. Supp.*, March 2, p. 10,967.

Immisch Electric Locomotive for Mines. (Ill.) *Elec. World*, March 16, p. 158.

The Birmingham Electric Locomotive. *Science*, March 29, p. 235; *Indust.*, March 8, p. 235. (Ill.) *West. Elecn.*, March 30, p. 174.

Electric Locomotives for Mines. (Lykens, Pa.) *Science*, March 15, p. 195; *Sci. Am. Supp.*, March 16, p. 11,002; *N. Y. Elec. Rev.*, March 2, p. 1.

An Electric Omnibus. (Ill.) *Sci. Am. Supp.*, March 2, p. 10,969.

The Insertion of Storage Batteries in Cars. (Corr.) R. M. Hunter. *Elec. World*, March 23, p. 175; P. P. Quackenboss, March 30, p. 189. (Corr.) Jno. A. Brill, March 9, p. 144.

New Brush for Railway Motors. *Lond. Elec. Rev.*, March 29, p. 355; *Md. Lt. & Ht.*, March 14, p. 346; *Elec. World*, March 9, p. 151; *West. Elecn.*, March 9, p. 138. (Thomson-Houston.)

PRIMARY BATTERIES, ETC.

Diffusion in Double Fluid Batteries. *Lond. Elec. Rev.*, March 15, p. 293.

The Redding Battery Cover. (Ill.) *Elec. World*, March 9, p. 148.

A Miniature Battery. (Ill.) *Elec. World*, March 16, p. 156. (Farnham.)

Bichromate Batteries for Domestic Electric Lighting. (Ill.) (Mareschal & Radiguet.) *Sci. Am. Supp.*, March 23, p. 11,022.

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The Resistance of Electrolytes to the Passage of Very Rapidly Alternating Currents. (Ill.) J. J. Thomson. *Lond. Elecn.*, March 22, p. 561.

SECONDARY BATTERIES, Etc.

New Rheostat for Seely Storage Battery. (Ill.) *N. Y. Elec. Rev.*, March 9, p. 8.

Pumpelly Storage Battery. (Note.) *Lond. Elec. Engr.*, March 22, p. 227.

The Detroit Storage Battery. (Ill.) *Sci. Am. Supp.*, March 9, p. 10,994.

Detroit Batteries for Street Car Propulsion. (Ill.) *West. Elecn.*, March 2, p. 126; March 23, p. 163.

Testing the Detroit Storage Battery for Traction. *Elec. World*, March 23, p. 176. "Tests of the Detroit Storage Battery." (Ill.) L. Duncan & G. A. Liebig, Jr., March 23, p. 177.

The Macraeon Secondary Battery. (Ill.) *Science*, March 15, p. 194.

Storage Battery Cabinets. (Ill.) *N. Y. Elec. Rev.*, March 9, p. 1.

The Knowles Method of Repairing Storage Battery Plates. *Elec. World*, March 16, p. 160.

Storage Batteries in Cars. (Note.) *Lond. Elec. Rev.*, March 22, p. 336; (J. A. Brill.)

Storage Batteries. *Md. Lt. & Ht.*, March 14, p. 346.

The Commercial Value of the Storage Battery. Foree Bain. *West. Elecn.*, March 23, p. 161.

The Storage Battery Plant in Potter Palmer's Residence. *N. Y. Elec. Rev.*, March 9, p. 3.

Recently Patented Improvements in Secondary Batteries. D. H. Bates. *Elec. World; Lond. Elec. Engr.*, March 1, p. 173.

Patent Rights in Secondary Batteries. *Elec. World; Lond. Elecn.*, March 1, p. 483.

Judge Coxe's Storage Battery Decision. *Elec. World*, March 30, p. 190. "The Meaning of the Coxe Storage Battery Decision," March 30, p. 194. "The Electrical Accumulator Company and the Coxe Decision." p. 194. *West. Elecn.*, March 30, p. 181.

Storage Battery Litigation. (Electrical Accumulator Company *vs.* Julien Electric Company) *Elec. World*, March 23, p. 171.

The Secondary Battery Decision. (Judge Coxe.) *N. Y. Elec. Rev.*, March 30, p. 9.

The Storage of Electricity. *Sci. Am. Supp*, March 2, p. 10,968.

ELECTROLYSIS.

The Mechanism of Electrolysis by Means of Alternating Currents. *N. Y. Elec. Rev.*, March 30, p. 1.

Spontaneous Explosions Occurring During the Electrolysis of Acidulated Water by Means of an Alternating Current. *Comptes Rendus; N. Y. Elec. Rev*, March 30, p. 8.

An Experiment in the Theory of Electrolysis by Prof. W. Ostwald, of Leipsig. (Corr.) Oliver Lodge. *Lond. Elecn.*, March 1, p. 493.

Mr. Webster's Electrolytic Method of Sewage Purification (Ill.) *Lond. Elecn.*, March 29, p. 588.

On Electrolysis. Violle & Chassagny. *Comptes Rendus; Lond. Elec. Rev.*, March 15, p. 294.

Some Peculiarities of Electro-Deposition. (Ill.) Alex. Watt. *West. Elecn.*, March 9, p. 141; March 16, p. 153.

The Electric Sewage Works at Crossness. (Ill.) *Lond. Elec. Engr.*, March 29, p. 249.

OVERHEAD AND UNDERGROUND WIRES.

Underground Conductors. *West. Elecn.*, March 9, p. 142.

Underground Wires in the District of Columbia, U. S. A. *Lond. Elec. Rev.*, March 22, p. 341.

Underground Wires in Minneapolis. *West. Elecn.*, March 23, p. 163.

Underground Mains. (Corr.) H. St. John Winkworth. *Lond. Elec. Engr.*, March 8, p. 193.

Disruptive Discharges in Lead Cables. (Ill.) C. H. Rudd. Nat. Elec. Lt. Assn. *Elec. World*, March 2, p. 126; *N. Y. Elec. Engr.*, March, p. 92; *Lond. Elec. Rev.*, March 15, p. 299. (Note.) *Lond. Elec. Engr.*, March 29, p. 248; *N. Y. Elec. Rev.*, March 9, p. 12.

Conduits, Their Material in Relation to Insulated Conductors. A. C. Chenoweth. Nat. Elec. Lt. Assn. *Elec. World*, March 2. p. 131; *N. Y. Elec. Engr*, March, p. 99; *Lond. Elec. Rev.*, March 22, p. 343.

Interesting Points on Overhead Wires. *N. Y. Elec. Rev.*, March 23, p. 6.

The Massachusetts Legislature and Electric Railway Wires in Boston. *Elec. World*, March 16, p. 164.

Safety of Overhead Wires Demonstrated. *Md. Lt. & Ht.*, March 14, p. 340.

New York Board of Electrical Control. *Elec. World*, March 2, p. 134; March 9, p. 149; March 16, p. 164; March 30, p. 194; *West. Elecn.*, March 9, p. 138; March 16, p. 150; March 30, p. 175.

INSTRUMENTS, MEASUREMENTS, ETC.

Alternating Current Ammeters. P. Nipkow. *Electrotechnische Zeitschrift; Elec. World*, March 9, p. 144.

On the Application of Clark's Cell to the Construction of a Standard Galvanometer. R. Threlfall. Phys. Soc. *Lond. Elec. Engr.*, March 29, p. 263.

A New Polarimeter. S. P. Thompson. Phys. Soc. *Indust.*, March 1, p. 210

The Hookham Electric Meter. (Ill.) *Elec. World*, March 9, p. 148.

Aron Electric Meters. (Ill.) *West. Elecn.*, March 9, p. 139.

The Holden Hydrometer. (Ill.) *Lond. Elecn.*, March 22, p. 578.

The Thomson Electric Meter. (Ill.) *Lond. Elec. Engr.*, March 22, p. 235.

Profs. Ayrton & Perry's Hot Wire Voltmeter. (Ill.) *Lond. Elec. Rev*, March 8, p. 272; *West. Elecn.*, March 23, p. 171.

Lockwood Ammeter and Polarity Indicator. (Ill.) *N. Y. Elec. Rev.*, March 23, p. 1.

Prize Competition of Electricity Meters. *Elec. World*, March 16, p. 160.

The Electrical Standardising Laboratory at Berlin. (Note.) *Indust*, March 29, p. 308.

On the Measurement of the Resistance of Imperfectly Purified Sulphur. Prof. Threlfall & A. Pollock. Phys. Soc. *Lond. Elec. Engr.*, March 29, p. 263.

On the Measurement of High Specific Resistances. R. Threlfall. Phys. Soc. *Lond. Elec. Engr.*, March 29, p. 263.

MISCELLANEOUS.

The "Victor" Split Insulator and Hanger. (Ill.) *Elec. World*, March 30, p. 189; *N. Y. Elec. Rev.*, March 30, p. 6; *West. Elecn.*, March 30, p. 172.

The Wimshurst Electric Machine. (Ill.) *N. Y. Elec. Rev.*, March 9, p. 2; *Elec. World*, March 9, p. 148.

Ball and Socket Insulating Joint. (Ill.) *West. Elecn.*, March 23, p. 163.

An Electric Valve. (Note.) M. Neyreneuf. *Journal de Physique; Lond. Elec. Engr.*, March 15, p. 206.

Electrical Type-Setter. (Note.) *Lond. Elec. Engr.*, March 8, p. 187.

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An Insulator for Electrical Conductors. W. C. Brown. *Sci. Am.*, March 30, p. 195.

An Electric Date Stamp. *Eng.; Science*, March 1, p. 160.

An Electrical Messenger. *Md. Lt. & Ht.*, March 14, p. 347. (New Haven Clock Co.)

A Novel Magnetic Watch Protector. (Ill.) *Elec. World*, March 9, p. 148.

Novel Electric Door-Opener. (Ill.) *Elec. World*, March 30, p. 190

Winding Clocks Electrically. *Elec. World*, March 16, p. 156.

Automatic Pole Lighting Arrester. (Ill.) *Elec. World*, March 9, p. 144.

Lightning Arrester. (C. H. Wilson.) *West. Elecn.*, March 23, p. 162.

The Fauber Bell (Ill.) *West. Elecn.*, March 23, p. 162.

A New Annunciator. (Ill.) (Edwards & Co.) *West. Elecn.*, March 9, p. 137.

Electrical Novelties. (Ill.) *West. Elecn.*, March 9, p. 137.

The Ostrander Door Opener. (Ill.) *N. Y. Elec. Rev.*, March 9. p 3; *West. Elecn.*, March 2, p. 125.

New Electrical Devices. *Md. Lt. & Ht.*, March 7, p. 315. (Elec. Supply Co.)

Berry's Improved Electric Bell. (Ill.) *Lond. Elecn.*, March 22, p. 574; *Indust.*, March 22, p. 281; *Lond. Elec. Rev.*, March 22, p. 325.

Electrical Exhibits and the Institute of Journalists. *Lond. Elec. Rev*, March 22, p. 324.

The Ice Carnival. (Note.) *Lond. Elec. Rev.*, March 15, p. 306.

Some Queer Joints. C. Monroe Doten. *Elec. World*, March 16, p. 161.

Mining Engineers in Convention. *Elec. World*, March 2, p. 134.

The Hamilton Plan for Utilizing Niagara. H. F. Watts. *Elec. World*, March 2, p. 133.

The Electric Duel. (Ill.) *Elec. World*, March 9, p. 149.

Electricity in Japan. *West. Elecn.*, March 30, p. 172.

The Copper Question. *N. Y. Elec. Rev.*, March 23, p. 4.

Welding by Electricity. (Note.) *Lond. Elec. Engr.*, March 29, p. 247; *West. Elecn.*, March 16, p. 149; *Md. Lt. & Ht.*, March 7, p 315. (Malden, Mass.) *N. Y. Elec. Rev.* (Ill.), March 23, p. 3.

The Electric Current. *N. Y. Elec. Rev.*, March 30, p. 4.

Electric Lighting Conductors by Means of Gas and Water Pipes. *N. Y. Elec. Rev.*, March 23, p. 6. Lightning Conductors. March 23, p. 7.

Electrical World Portraits. III. Prof. Henry A. Rowland. *Elec. World*, March 9, p. 142. **Leo. Daft. IV.** March 30, p. 184.

S. A. Barton. (With Portrait.) *West. Elecn.*, March 16, p. 148.

Edwin Ruthven Weeks. (With Portrait.) *West. Elecn.*, March 2, p. 123.

The Canadian Electrical Society of Montreal. *Elec. World*, March 2, p. 135.

"Light and Color." Prof. Robt. Spice. Elec. Club. *N. Y. Elec. Rev*, March 30, p. 2; *Md. Lt. & Ht.*, March 28, p. 402.

Chicago Electric Club. *West. Elecn.*, March 16, p. 148; March 30, p 173.

The Boston Electric Club. *Md. Lt. & Ht.*, March 14, p. 341

The Centennial of Ohm. (With Portrait.) *Elec. World*, March 16 p. 156; *Lond. Elec. Rev.*, March 1, p. 240.

Annual Meeting of the New York Electrical Society. *Elec. World*, March 30, p. 189; *West. Elecn.*, March 30, p. 181

The Rose Polytechnic Institute. *Elec. World*, March 2, p 134.

Electrical Engineering at Cornell University. *N. Y. Elec. Rev.*, March 9, p. 7.

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NAME.	ABBREVIATION.	PUBLISHED, &C.
Engineer.	*Engr.*	London, 163 Strand, weekly. £1.9.
Engineering.	*Eng.*	London, 35–36 Bedford St W. C.; weekly. £1.16.
Industries	*Indust.*	Manchester, 70 Market St., weekly. £1.12.
The Electrician.	*Lond. Elecn.*	London, 1 Salisbury Court, E. C.; weekly. £1 2.
The Telegraphic Journal and Electrical Review.	*Lond. Elec. Rev.*	London, 22 Paternoster Row, E. C.; weekly. £1.1.8.
The Electrical Engineer	*Lond. Elec. Engr.*	London, 139–140 Salisbury Court; weekly, 17s. 4d.
Nature.	*Nat.*	London, 29 Bedford St., W. C.; weekly, £1.8.
The Electrical Engineer	*N. Y. Elec. Engr.*	New York, 11 Wall Street; monthly, $3
The Electrical World.	*Elec. World.*	New York, 168–177 Potter Building; weekly. $3
The Electrical Review.	*N. Y. Elec. Rev.*	New York, 13 Park Row; weekly. $3.
The Electric Age.	*Elec. Age.*	New York, 5 Dey Street; semi-monthly, $1.50.
Modern Light and Heat.	*Md Lt. and Ht.*	Boston, 146 Franklin Street; weekly, $3.
Western Electrician.	*West. Elecn.*	Chicago, 6 Lakeside Building; weekly, $3.
Electric Power.	*Elec. P'wr.*	New York, 150 Broadway; monthly, $3.
The Electro-Mechanic.	*Elec. Mech.*	Kansas City, Mo; semi-monthly, $2.
Practical Electricity.	*Prac. Elec.*	Boston, 620 Atlantic Ave.; monthly, $1.
Journal of the Franklin Institute.	*Jour. Fkln. Inst.*	Philadelphia, Franklin Institute; monthly, $5.
Science.	*Science.*	New York, 47 Lafayette Place, weekly, $3.50.
Scientific American.	*Sci. Am.*	New York, 361 Broadway; weekly, $3.
Scientific American Supplement.	*Sci. Am. Supp.*	New York, 361 Broadway; weekly, $5.
Mechanics.	*Mech.*	Philadelphia, 907 Arch St.; monthly, $1.
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Street Railway Journal	*St. Ry. Jour.*	New York, 113 Liberty St.; monthly, $2.
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Transactions of the Institute Already Published.

Vol. I, 1884.

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Vol. II, 1885.

THE PROTECTION OF ELECTRICAL INVENTIONS. F. Jastrow, of Washington, D. C.—COST OF MINE LIGHTING. Frank P. Mills, now of Ishpeming, Mich.—THE ELECTRIC ARC LAMP FRICTIONAL CLUTCH. (Illustrated.) By Edward P. Thompson of New York.—DESCRIPTION OF A LARGE SECONDARY BATTERY PLANT FOR GENERAL COMMERCIAL DISTRIBUTION. E. P. Roberts, of Cleveland, O.—DELANY'S SYSTEM OF FAC-SIMILE TELEGRAPHY. (Illustrated.) Prof. Edwin J. Houston of Philadelphia.

Vol. III, 1886. (Out of Print.)

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Vol. IV, 1887.

ELECTRIC STREET CARS. T. C. Martin, of New York.—THE PRACTICAL REQUIREMENTS OF SMALL MOTORS. S. S. Wheeler, of New York.—DISTRIBUTION OF ELECTRICITY BY SECONDARY GENERATORS. Discussion by F. L. Pope, Dr. Otto A. Moses, Captain O. E. Michaelis, Prof. Elihu Thomson and others.—STORAGE BATTERIES IN PRACTICE. E. P. Roberts, of Cleveland, O.—DIFFERING TEMPERATURE COEFFICIENTS OF THE DIFFERENT COILS OF A FINE RHEOSTAT. Prof. W. A. Anthony, now of Manchester, Conn.—ON THE BEST RATIO OF THE SECTION OF THE GRAMME ARMATURE TO THE SECTION OF THE FIELD MAGNETS. (Illustrated.) Dugald C. Jackson, now of Lincoln, Neb.—NOVEL PHENOMENA OF ALTERNATING CURRENTS. (Illustrated.) Prof. Elihu Thomson, of Lynn, Mass.—A FEW COMPARATIVE STATISTICS OF ELECTRIC RAILWAYS. T. C. Martin, of New York. (With tabular statement.)—CENTRAL STATION CONSTRUCTION. J. A. Powell, of Troy, New York.—ON THE CHANGE FROM NEGATIVE TO POSITIVE OF THE TEMPERA

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Vol. V, 1887-8.

ON ELECTRIC STREET CARS, WITH SPECIAL REFERENCE TO METHODS OF GEARING. Anthony Reckenzaun, of London. No. 1, October, 1887—A COULOMB METER, OR INSTRUMENT FOR MEASURING THE CONSUMPTION OF ELECTRICITY. (Illustrated.) Prof. George Forbes, F.R.S., of London. No. 2, November, 1887—RECENT IMPROVEMENTS IN APPARATUS FOR OCEAN CABLING (Illustrated.) Charles Cuttriss. Supplementary Note on the Siphon Recorder, and Ocean Telephony, by Thos D. Lockwood. No 3, December, 18 7—PHENOMENA OF RETARDATION IN THE INDUCTION COIL. (Illustrated.) William Stanley, Jr. No. 4, January, 1888.—REVISION OF THE PATENT LAW. Arthur Stewart, Esq. No. 5, February, 1888.—ELECTRIC ENERGY FROM CARBON WITHOUT HEAT. Willard E. Case, Auburn, N. Y. No. 6, March, 1888.—ALTERNATING CURRENT ELECTRIC MOTORS. Dr. Louis Duncan. Baltimore, Md. No. 7, April, 1888.—MAXIMUM EFFICIENCY OF INCANDESCENT LAMPS. (Illustrated.) John W. Howell. No. 8, May, 1888.—PROTECTION OF THE HUMAN BODY FROM DANGEROUS CURRENTS. Patrick B. Delany.—THE POSSIBILITIES AND LIMITATIONS OF CHEMICAL GENERATORS OF ELECTRICITY. Francis B. Crocker. No. 9, June, 1888—ON COMPENSATED RESISTANCE STANDARDS. (Illustrated.) Edward L. Nichols. A NEW SYSTEM OF ALTERNATE CURRENT MOTORS AND TRANSFORMERS. (Illustrated.) Nikola Tesla. UNDERGROUND ELECTRICAL CONDUCTORS IN EUROPE AND AMERICA. Prof. G. W. Plympton. THE PATENT COURT AND UNIFORMITY IN PATENT OFFICE PRACTICE. George H. Stockbridge. A SWINGING ARM GALVANOMETER. (Illustrated.) George S. Moler. No. 10, July, 1888. THE SOLUTION OF THE MUNICIPAL RAPID TRANSIT PROBLEM. (Illustrated.) Frank J. Sprague. No. 11, August, 1888. SOME OBJECTIONS TO THE OVERHEAD CONDUCTOR FOR ELECTRIC RAILWAYS, M. B. Leonard. NOTE ON GEARING FOR ELECTRIC RAILWAY MOTORS, Alfred Robinson. No. 12, September, 1888.

Vol. VI, 1889.

[Including October, November and December, 1888.]

THE GEYER-BRISTOL METER FOR DIRECT AND ALTERNATING CURRENTS. [Illustrated.] Prof. William E. Geyer. THE ABDANK MAGNETIC CALL AND THE ABDANK INTEGRAPH. [Illustrated.] E. Abdank-Abakanowicz. No. 1, January, 1889. SIX YEARS PRACTICAL EXPERIENCE WITH THE EDISON CHEMICAL METER. (Illustrated.) W. J. Jenks. No. 2, February, 1889. LIGHTNING ARRESTERS AND THE PHOTOGRAPHIC STUDY OF SELF-INDUCTION. (Illustrated.) E. G. Acheson. No. 3, March, 1889. A NEW SYSTEM OF MULTIPLEX TELEGRAPHY. (Illustrated.) Lieut. F. Jarvis Patten. LIGHTNING ARRESTERS AND THE PHOTOGRAPHIC STUDY OF SELF INDUCTION. Notes by Jo. Stanford Brown and Chas. T. Child. REPLY TO THE NOTES OF MESSRS. BROWN AND CHILD by E. G. Acheson. No. 4, April, 1889. THE EFFICIENCY OF METHODS OF ARTIFICIAL ILLUMINATION. Illustrated. Edward L. Nichols. No. 5, May, 1889. SOME RESULTS WITH SECONDARY BATTERIES IN TRAIN LIGHTING. (Illustrated.) Alexander S. Brown. THE INHERENT DEFECTS OF LEAD STORAGE BATTERIES. (Illustrated.) DR. Louis Duncan. ELECTRIC MOTOR REGULATION. (Illustrated.) Francis B. Crocker. Nos. 6 and 7, June and July 1889, (Double Number). MAGNETISM IN ITS RELATION TO INDUCED ELECTROMOTIVE FORCE AND CURRENT. (Illustrated.) Prof. Elihu Thomson of Lynn, Mass. ON THE RELATION BETWEEN THE INITIAL AND THE AVERAGE EFFICIENCY OF INCANDESCENT LAMPS. (Illustrated.) W. H. Peirce of New York. THE EFFICIENCY OF THE ARC LAMP. (Illustrated.) H. Nakano, of Japan, with an introductory note by Prof. E. L. Nichols. THE SPIRAL COIL VOLTAMETER. (Illustrated.) Harris J. Ryan, of Ithaca, N. Y. THE PERSONAL ERROR IN PHOTOMETRY. (Illustrated.) Prof. Edward L. Nichols, of Ithaca, N. Y. ON MODERN VIEWS WITH RESPECT TO ELECTRIC CURRENTS. (Illustrated.) Prof. Henry A. Rowland, of Baltimore, Md. CLASSIFIED LIST OF MEMBERS. Nos. 8 and 9, August and September, 1889, (Double Number.)

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